Before the
Federal Communications Commission
Washington, D.C. 20554

In the Matter of	)	
International Comparison and Consumer Survey Requirements in the Broadband Data Improvement Act	)	GN Docket No. 09-47
A National Broadband Plan for Our Future	)	GN Docket No. 09-51
Deployment of Advanced Telecommunications Capability to All Americans in a Reasonable and Timely Fashion and Possible Steps to Accelerate Such Deployment Pursuant to Section 706 of the Telecommunications Act.	)	GN Docket No. 09-137

DECLARATION OF ROBERT W. CRANDALL, EVERETT M. EHRLICH AND JEFFREY A. EISENACH REGARDING THE BERKMAN CENTER STUDY (NBP PUBLIC NOTICE 13)

NOVEMBER 16, 2009

CONTENTS

I. INTRODUCTION

1. We have been asked by National Cable & Telecommunications Association and United States Telecom Association[1] to review and comment on *Next Generation Connectivity: A Review of Broadband Internet Transitions and Policy from Around the World,* a draft report submitted to the Federal Communications Commission by the Berkman Center for Internet and Society.[2]

2. Our declaration responds specifically to National Broadband Plan Notice #13,[3] in which the Commission asked for comment on six specific issues:

> *1. Does the study accomplish its intended purposes?*
> *2. Does the study provide a complete and objective survey of the subject matter?*
> *3. How accurately and comprehensively does the study summarize the broadband experiences of other countries?*
> *4. How much weight should the Commission give to this study as it develops a National Broadband Plan?*
> *5. Are additional studies needed along the lines of the Berkman Study?*
> *6. Please provide any other comments on the Berkman Study that you deem relevant.*

3. We conclude that the *Berkman Study* does not provide a complete or objective survey of the subject matter, nor does it present an accurate or comprehensive summary of the evidence regarding broadband policies in other countries. The study does not serve the Commission's announced purpose of obtaining an "independent expert review of existing

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[1] The opinions expressed herein are our own. This research was requested and supported by NCTA and USTA. We are grateful to Allan Ingraham, Kevin Caves and Andrew Card for assistance. We also thank John de Ridder for providing us with the regression data he used in his 2007 OECD study.

[2] *Next Generation Connectivity: A Review of Broadband Internet Transitions and Policy from Around the World,* October 2009 (Draft) (available at http://www.fcc.gov/stage/pdf/Berkman_Center_Broadband_Study_13Oct09.pdf) (hereafter, *Berkman Study*).

[3] Federal Communications Commission, Comments Sought on Broadband Study Conducted by the Berkman Center for Internet and Society, (DA 09-2217, October 14, 2009).

literature and studies about broadband deployment and usage throughout the world."[4] Rather, the study appears to be explicitly biased towards a particular policy agenda, advancing findings and conclusions that conflict with the accumulated evidence found in existing research. The study's attempt to provide new evidence that contradicts this mounting body of evidence fails badly. Accordingly, the Commission should not rely on the *Berkman Study's* findings in developing its National Broadband Plan.

4. Specifically and most importantly, the Commission should reject what the *Berkman Study* refers to as its "most surprising and significant" finding, namely that

> "open access" policies – unbundling, bitstream access, collocation requirements, wholesaling, and/or functional separation – are almost universally understood as having played a core role in the first generation transition to broadband in most of the high performing countries; that they now play a core role in planning for the next generation transition; and that the positive impact of such policies is strongly supported by the evidence of the first generation broadband transition.[5]

We agree with the *Berkman Study* that such a finding – if it were supported by the evidence – would indeed be both "surprising" and "significant." Wishing, however, cannot make it so: the incontrovertible fact is that open access policies have not been shown to increase broadband adoption, availability, or infrastructure investment. To the contrary, the bulk of the available evidence points in the opposite direction, and the *Berkman Study* provides no new evidence that supports a different conclusion.

5. The remainder of this declaration is organized as follows. In Section II, we briefly present our qualifications. In Section III, we discuss the *Berkman Study's* "review of existing literature and studies about broadband deployment and usage," including its over-

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[4] Federal Communications Commission, "Harvard's Berkman Center to Conduct Independent Review of Broadband Studies to Assist FCC" (Press Release, July 14, 2009).

[5] *Berkman Study* at 11.

reliance on a single, fatally flawed regression analysis to support its conclusions and its failure to mention, let alone comprehensively review, much of the available research on the impact of unbundling policies in the U.S. and around the world. In Section IV, we examine the *Berkman Study's* "case studies" of broadband policies in selection countries, and find them to be in many cases factually inaccurate, generally misleading, and, overall, unsupportive of the report's conclusions. In Section V, we point out various other errors and omissions in the *Berkman Study*, which taken together demonstrate further that the report's conclusions cannot be relied upon as the basis for decisions by the Commission. In Section VI, we present a brief summary of our conclusions.

II. QUALIFICATIONS

6. My name is Robert W. Crandall. I am a Nonresident Senior Fellow in Economic Studies at the Brookings Institution in Washington, DC, where I have been since 1978. Prior to that I was the Acting Director, Deputy Director, and Assistant Director of the Council of Wage and Price Stability in the Executive Office of the President, and between 1974 and 1975 I was an Adviser to Commissioner Glen Robinson of the Federal Communications Commission. I was an Assistant Professor and Associate Professor of Economics at MIT between 1966 and 1974. I have written widely on telecommunications policy, the economics of broadcasting, and the economics of cable television. I am the author or co-author of eight books on communications policy published by the Brookings Institution, including *Competition and Chaos: U.S. Telecommunications since the 1996 Act* (2005) and *Broadband: Should We Regulate High-Speed Internet Access?* (edited with James Alleman, 2002). My complete *curriculum vita* is provided as Exhibit A to this declaration.

7. My name is Everett M. Ehrlich. I am President of ESC Company, an economics consulting firm in Washington, DC. I have served as the Undersecretary of Commerce for Economic Affairs (Clinton I) where, among other duties, I co-chaired (with W. Bowman Cutter) the National Economic Council's Interagency Group on the structural effects of technological change on the economy. I have also been the Chief Economist and later head of strategic planning for Unisys Corporation, then a Fortune 100 supplier of computer hardware and systems. I have also served as Senior Vice-President and Director of Research of the Committee for Economic Development, a nonpartisan, business-based public policy think tank, and as Assistant Director of the Congressional Budget Office for Natural Resources and Commerce. I hold a Ph.D. in economics from the University of Michigan and a B.A. in economics from the State University of New York at Stony Brook. My biography is provided as Exhibit B to this declaration.

8. My name is Jeffrey A. Eisenach. I am Chairman of Empiris LLC, an economic consulting firm based in Washington, D.C., and an Adjunct Professor at George Mason University Law School. I have served in senior policy positions at the U.S. Federal Trade Commission (FTC) and the White House Office of Management and Budget (OMB), and on the faculties of Harvard University's Kennedy School of Government and Virginia Polytechnic Institute and State University. Prior to joining Empiris, I served as Chairman of Criterion Economics, Chairman of CapAnalysis, the economic consulting arm of Howrey LLC, and President of The Progress & Freedom Foundation. I have authored or co-authored numerous expert reports in litigation matters as well as in regulatory proceedings, testified before Congress on multiple occasions, and am the author or co-author of eight books. I hold a Ph.D.

in economics from the University of Virginia and a B.A. in economics from Claremont McKenna College. My complete *curriculum vita* is provided as Exhibit C to this declaration.

III. THE *BERKMAN STUDY'S* CONCLUSIONS ARE NOT SUPPORTED BY QUANTITATIVE ANALYSIS

9. The *Berkman Study* focuses on two market characteristics associated with broadband diffusion: the intensity of competition and the rate of investment. Unfortunately, it focuses almost exclusively on *intra-platform* competition and *public* investment. It discounts the significance of *platform competition* and of *private* investment, allowing its authors to ignore the link between policies that promote private investment in competitive platforms, on the one hand, and market performance, on the other.

10. The *Berkman Study* claims that its attachment to intra-platform competition through mandated network unbundling of incumbent telecommunications networks at regulated prices is grounded in empirical analysis. However, it largely ignores the vast empirical literature that finds no general relationship between network unbundling and broadband penetration, while focusing on one study, by John de Ridder,[6] which appears to find such a relationship. Based on its re-estimation of de Ridder's original specifications, the *Berkman Study* finds that mandatory unbundling policies increase broadband penetration by one percent per year. As we explain in Section III (A) below, however, subsequent research demonstrated that de Ridder's results are spurious; and, our further econometric analysis demonstrates that the *Berkman Study's* efforts to resuscitate de Ridder's results are unsuccessful. Simply put, the quantitative analysis upon which the *Berkman Study* bases its conclusion that unbundling increases broadband penetration is invalid.

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[6] John de Ridder, "Catching-up in Broadband – What Will it Take?" *Organisation for Economic Co-Operation and Development* (July 25, 2007) (hereafter, *de Ridder Study*).

11. Given the *Berkman Study's* emphasis on unbundling and its mandate to "review existing literature and studies about broadband deployment and usage," we were surprised at its cursory treatment of the extensive existing literature on the effects of unbundling regulation, which on balance fails to support the proposition that unbundling enhances penetration, but does find that unbundling reduces investment incentives, especially for new technologies such as fiber to the premises.[7]

12. In section III (B) below, we conduct the broader review of the existing literature we would have expected to find in the *Berkman Study*. Our review demonstrates that the bulk of the existing quantitative research finds no basis for concluding that unbundling leads to higher levels of broadband penetration, increased infrastructure investment, or other positive effects on significant public policy objectives.

A. The *Berkman Study's* "Do-Over" of the *de Ridder Study* is Fatally Flawed

13. In this section, we review the *Berkman Study's* econometric analyses, from which it concludes that unbundling policies increase broadband penetration. These analyses are based on the 2007 OECD *de Ridder Study*. Based on its econometric analysis, the *Berkman Study* concludes that

> consistent with the findings of this recent work, and inconsistent with a recent critique of it, econometric analysis supports the proposition that unbundling contributed to broadband penetration in OECD countries. Indeed, new analyses we perform on the existing data suggest that the effect was larger than previously thought, the confidence level higher, and the finding more robust.[8]

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[7] The United States has been a leader in network investment, in large part as a result of its rejection of overly broad unbundling policies, but the *Berkman Study* chooses to ignore this encouraging result, perhaps because it conflicts with its policy recommendations. See, e.g., Jeffrey A. Eisenach, "Broadband in the U.S. – Myths and Facts," in *Australia's Broadband Future: Four Doors to Greater Competition* (Melbourne: Committee for Economic Development of Australia, 2008) 48-59.

[8] *Berkman Study* at 75.

As we demonstrate below, the Berkman Study's econometric analysis does not support any of these conclusions.

14. The *de Ridder Study* presents least-squares regression results for a model that takes the general form:

$$Qtot_{it} = a_0 + a_1 GUyrs_{it} + \sum_{j=1}^{n} b_j X_{j,it} + e_{it},$$

where:

Qtot represents total broadband penetration (as measured by the ratio of broadband connections to population in each country);

GUyrs represents the number of years since unbundling policies were enacted in each country;

X represents a vector of other explanatory variables (denoted by the index variable ***j***) such as urbanization, average age, or median income;

i is an index variable representing the country and ***t*** is an index variable representing time;

a and ***b*** are regression coefficients; and

e is a random and identically distributed error term that captures variance unexplained by the model.[9]

15. Simply put, the thesis being tested is that, holding other factors constant, unbundling policies increase broadband penetration, with the effect increasing with the length of time unbundling policies have been in place. Thus, de Ridder estimates least squares regression parameters for various regression specifications to determine whether the coefficient on the unbundling variable (a_1 in the equation above) is positive and statistically significant.

16. Specifically, the *de Ridder Study* estimates seven different regression equations that include the *GUyrs* policy variable. Four of those seven equations yield a positive

[9] For ease of exposition, we utilize the same system of notation as that used by both the *de Ridder Study* and the *Berkman Study*.

coefficient for this variable that is significant at the 5 percent level. On this basis, de Ridder concludes that "unbundling (as measured by *GUyrs*) is currently more significant than platform competition in explaining broadband penetration."[10] Obviously, this result supports the *Berkman Study's* pro-unbundling position.

17. However, the *de Ridder Study* was effectively critiqued in a 2008 paper by Boyle, Howell and Zhang.[11] By estimating de Ridder's regressions with robust standard errors, their paper demonstrates that the statistical significance of *GUyrs* disappears after controlling for heteroskedasticity. In addition, Boyle, Howell, and Zhang showed that *GUyrs* was likely serving as a proxy for the natural diffusion of broadband into the economy – that is, rather than capturing the time elapsed since the adoption of *unbundling*, GUyrs was actually capturing, at least in part, the time elapsed since the *introduction of DSL*.[12] Indeed, when Boyle, Howell, and Zhang controlled for the number of years broadband service had been available in each country, the *GUyrs* variable became insignificant in both an economic and statistical sense.[13]

18. The *Berkman Study* seeks to resuscitate the de Ridder results by making two significant modifications. First, it modifies the *GUyrs* variable, either by replacing it with a zero-one indicator variable for unbundling, or by replacing *the values* of *GUyrs* used by de

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[10] *de Ridder Study* at 20.

[11] Glenn Boyle, Bronwyn Howell and Wei Zhang, "Catching up in Broadband: Does Local Loop Unbundling Really Lead to Material Increases in OECD Broadband Uptake?" *New Zealand Institute for the Study of Competition and Regulation Working Paper* (July 2008) (hereafter *Boyle, Bronwyn and Zhang*).

[12] The process by which new technologies are adopted has been studied by economists for many years. *See, e.g.*, Zvi Griliches, "Hybrid Corn: An Exploration in the Economics of Technological Change," 25 *Econometrica,* Oct. 1957 (showing the standard technology diffusion curves in the case of the introduction of hybrid corn into farming).

[13] *Boyle,Howell and Zhang* at 7-9. Boyle, Howell and Zhang also corrected various other problems in the *de Ridder Study,* including de Ridder's misspecification of the *GUyrs* variable, which he allowed to take negative values in the years prior to the adoption of unbundling.

Ridder with new values.[14] Second, it applies a more sophisticated econometric technique, utilizing a mixed-effects regression model.[15] Based on these changes, the *Berkman Study* concludes that unbundling, whether represented by an indicator variable or a continuous time variable, is generally significant and positive, i.e., that unbundling increases broadband penetration.[16]

19. The *Berkman Study's* econometric analysis is flawed for three primary reasons. First, it simply fails to address the Boyle-Howell-Zhang critique of the *de Ridder Study*, which showed that the *GUyrs* variable is capturing the natural diffusion of broadband over time rather than the effect of unbundling. Second, the mixed effects regressions presented in the *Berkman Study* fail a Hausman Test for specification, implying that the mixed effects model is inappropriate for this data set and does not produce reliable results. Third, its decision to simply replace the values of the key variable (*GUyrs*) with values more consistent with its prior beliefs is simply unjustifiable as a matter of econometric technique.

20. In what follows, we present the results of our own regression analysis of the basic de Ridder/Berkman model. Our regressions utilize additional years of data (thus significantly increasing the number of observations, as well as utilizing more recent observations), and apply more appropriate regression specifications and techniques. As we

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[14] Like *Boyle,Howell and Zhang,* the *Berkman Study* also eliminates negative values for *GUyrs*.

[15] The *de Ridder Study* performs least-squares regressions on a 30 country sample from 2005, and a pooled 54 observation sample by adding 24 data points from 2002; it also differenced the two data sets and performed regressions on the resulting 24-country sample. The regressions on the differenced data, however, did not include the *GUyrs* variable, as differencing it would result in a constant of 3 across all observations, which would result in the dropping of the variable from the regression. That is, one cannot estimate a regression parameter for a "variable" that does not vary.

[16] *Berkman Study* at 117. The *Berkman Study* presents regression results for approximately two dozen different model specifications, some of which simply replicate the results in the *de Ridder Study,* and the remainder of which estimate least-squares and mixed effects models using different regression specifications and different formulations of the *GUyrs* variable.

demonstrate, when additional years of data and additional relevant explanatory variables are added – that is, when the model is more fully and correctly specified, and applied to a more complete data set – the effect of unbundling not only disappears but reverses: Rather than increasing broadband penetration, our results demonstrate that unbundling reduces it.

1. The Inclusion of Relevant Data and Explanatory Variables Reverses the Effect of Unbundling on Penetration

21. The primary reason for the *de Ridder Study's* decision to limit observations to these two years was its use of regressions that include the price of DSL service, for which data is only available for 2002 and 2005. Perhaps because it initially focuses on replicating the de Ridder regressions, the *Berkman Study* also uses only data from 2002 and 2005.

22. However, as the *Berkman Study* explains, because the price of DSL service is correlated with the unbundling variable,[17] it is not appropriate to use price as a separate explanatory ("right-hand-side") variable. Accordingly, in most of its regression specifications, the *Berkman Study* drops the DSL price variable, which would have allowed it to utilize additional years of data. As we explain further below, our regression analyses rely on data from 2001-2006, for a total of 168 observations.

a. Least-Squares Regression Results

23. To examine the effect of both increases in sample size and the natural diffusion of broadband into the economy, we perform regression analysis on fourth quarter OECD data from 2001 through 2006. Table 1 lists the variables used in the analysis and presents summary

[17] *Berkman Study* at 115, 141.

statistics. The regression data reflects a 28-country (rather than 30-country) sample, because we were unable to determine the date of DSL availability in Greece and the Slovak Republic.[18]

Table 1:
Summary Statistics for Regression Data of 28 OECD Countries (168 Observations)

Variable	Description	Mean	Standard Deviation	Minimum	Maximum
Qtot	Broadband Penetration	9.976	8.497	0.010	31.788
GUyrs	Years since unbundling enacted	3.940	2.942	0.000	11.000
unbundled	=1 if guyrs > 0 = 0 otherwise	0.839	0.368	0.000	1.000
DSLyears	Time since DSL was available	4.238	1.813	0.166	7.667
Pop dens	Population density	139.838	133.943	2.527	489.183
Pops	Population in millions	40.719	58.814	0.287	299.715
Gdp	Gross Domestic Product per capita in US PPP	28720.140	11688.920	6178.182	80471.400
State owned	= 1 if network was state owned = 0 otherwise	0.071	0.258	0.000	1.000

It should be noted that the simple correlation coefficient between variables *GUyrs* and *DSLyears* is 0.58. This indicates that, as expected, the two variables share a moderate to strong positive correlation.

24. Table 2 below presents least squares regression results of "reduced form" specifications[19] that include *DSLyears*, and estimate the effect of unbundling using either the *GUyrs* variable or, in some specifications, *Unbundled*, which is a one-zero indicator variable.

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[18] Our regression data are presented in Exhibit D, so that others can replicate our results and also perform original analysis using the data. We also note that more recent OECD reports have updated certain data series. To be clear, we have used broadband penetration data from Table 1G of the OECD's December 2008 Broadband Report. These data are available from the OECD Broadband Portal, at http://www.oecd.org/document/54/0,3343,en_2649_34225_38690102_1_1_1_1,00.html. Prior series are available in the December reports for various years, which are available at http://www.oecd.org/statisticsdata/0,3381,en_2649_34225_1_119656_1_1_1,00.html.

[19] The exclusion of price in the equations is appropriate because price is an endogenous variable that is likely correlated with the disturbance term in the *Qtot* regression.. Under a linear two equation model with *Qtot* and *Price* as dependent variables, the *Price* equation would serve as the cost equation, with the right hand side variables that affect the build out and operation of the broadband network. Population density, for example, would be a natural cost shifter that would serve this purpose. Substituting for price in the *Qtot* equation then yields the

Table 2:
Least-Squares Estimates with *Qtot* as the Dependent Variable

Variable	Coefficient	Robust t-Stat	Coefficient	Robust t-Stat
	Specification 1		Specification 2	
GUyrs	0.339	1.460	—	—
Unbundled	—	—	-2.232**	-1.980
DSLyears	2.900***	10.210	3.253***	16.850
pop dens	0.012***	3.190	0.013***	3.480
Pops	-0.017***	-4.030	-0.013***	-3.560
Gdp	0.000***	5.060	0.000***	7.710
state owned	-7.400***	-4.260	-9.602***	-6.170
Constant	-10.042***	-7.430	-10.544***	-9.170
	N = 168		N = 168	
	R-squared = 0.72		R-squared = 0.72	
	F (zero slopes) = 83.94		F (zero slopes) = 76.41	

Note 1: For both specifications, a White test for heteroskedasticity rejects the null hypothesis of homoskedasticity. We therefore present the regression results with t-Statistics generated from White-Huber standard errors, which account for correlation between the right hand side variables and the error terms in the regression.
Note 2: ***Denotes significance at 1 percent; ** denotes significance at 5 percent.

25. To begin, the regression results in Table 2 first show that the explanatory variables in the model are, in general, statistically significant at a 1 percent significance level. Specifically, the coefficient on *DSLyears* is positive and significant in both regression specifications, and positive coefficients on population density and GDP indicate that broadband penetration tends to be higher in wealthier economies and in countries with higher population densities, but lower in larger countries (those with large populations) and in countries where the telecommunications network is state-owned. All of these effects are of the anticipated sign.

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reduced form equation, which excludes *Price*, but still controls for its affect. An alternative approach would be to instrument for *Price* using two-stage least squares. Additional data on price over time would be required to perform this correction.

26. Regarding unbundling, the *Berkman Study* found that the effect of *GUyrs* was positive and statistically significant at 1 percent. But when *DSLyears* is inserted into the equation, as in Specification 1, the effect disappears, indicating that the number of years unbundling policies have been in place has no independent effect on broadband penetration, and confirming the result in Boyle-Howell-Zhang that *GUyrs* is simply a proxy for the number of years since DSL was first introduced.[20]

27. As explained above, the correct question with respect to unbundling is whether it shifts the technology diffusion curve, so that, other things equal, broadband penetration increases more rapidly in countries that adopt unbundling. To test this proposition, we introduce the indicator variable *Unbundled* in a regression that also controls for the time that DSL has been available in each country. The second regression specification in Table 2 provides the results of this test: as the table shows, the coefficient on *Unbundled* is *negative* and statistically significant.[21] This result indicates that unbundling has *slowed* the pace of broadband adoption in the sample countries, a result which directly contradicts the *Berkman Study*, but, as we note below, is consistent with prior empirical research.

b. Fixed Effects, Random Effects, and Generalized Least Squares

28. We next extend the above regressions to the estimation of fixed effects, random effects, and generalized least-squares models. A fixed effects regression essentially involves the inclusion of country-specific indicator variables in the model, and is a common practice in panel datasets. Random effects regressions take advantage of variation across countries without the

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[20] Our confidence in this conclusion is further strengthened by the fact that, if we replicate the *Berkman Study's* misspecification, we get the same result: When *DSLyears* is removed from our regression, *GUyrs* becomes positive and statistically significant at 1 percent.

inclusion of indicator variables for each country in an attempt to estimate the right hand side variables in a more efficient manner than fixed effects. Applying generalized least squares to panel data allows one to control for both heteroskedasticity (a cross-sectional data problem) and autocorrelation (a time-series problem).

Table 3:
Fixed Effects, Random Effect, and GLS Regression Results with *Qtot* as the Dependent Variable

Variable	Coefficient	t-Stat	Coefficient	Robust t-Stat	Coefficient	Z-Stat
	Fixed Effects		Random Effects		GLS	
Unbundled	-3.7421***	-3.8	-3.8146***	-3.91	-1.2038*	-1.93
DSLyears	2.5213***	8.62	3.0633***	19.24	2.2488***	13.54
pop dens	-0.1093	-0.68	0.0138*	1.94	0.0009	0.08
Pops	-0.5699***	-2.89	-0.0137	-0.85	-0.0296**	-2.34
Gdp	0.0009***	5.24	0.0004***	4.7	0.0007***	6.31
Constant	14.199	0.6	-11.351***	-4.64	-17.321***	-5.06
	N = 168		N = 168		N = 168	
	R-squared (within) = 0.88		R-squared (within) = 0.86		Chi-Squared = 1367.86	
	R-squared(between) = 0		R-squared(between) = 0.33			
	R-Squared (overall) = 0.01		R-Squared (overall) = 0.61			

29. The results reported in Table 3 show that the *Unbundled* variable has a negative coefficient when any of these panel data techniques is used. In the case of fixed and random effects, the *Unbundled* variable is negative and statistically significant at 1 percent. In the GLS model controlling for both autocorrelation[22] and heteroskedasticity, *Unbundled* is again negative, and significant at 5.4 percent. Therefore, panel data analysis supports the least-squares results presented in Table 2 above – namely, that unbundling reduces broadband penetration.

[22] An AR1 process was estimated, yielding a constant autocorrelation coefficient of 0.99 for all countries. There were 28 separate estimated covariances for the heteroskedasticity correction. Relaxing the heteroskedasticity correction yields similar results. The population variable, however, is insignificant, and the p-value for the *Unbundled* variable is 0.08.

30. It is also worth noting that in the fixed effects regression, the effect for the United States is both positive and statistically significant. That is, the fixed effect regression indicates that the United States has broadband penetration above what the model predicts based on the other control variables in the model, such as wealth and population density. Moreover, the United States fixed effect is statistically significant at 1 percent both in regressions that apply traditional standard errors and in regressions that use robust heteroskedastic-consistent standard errors. This finding indicates that a characteristic not specifically controlled for in the model is causing higher broadband penetration in the U.S. than the model itself predicts. Plausible candidates for such a characteristic include tastes (i.e. consumer preferences), the presence of robust infrastructure competition, or policy variables (other than unbundling) not captured in the model.

2. The Mixed Effects Regressions in the *Berkman Study* Fail a Hausman Specification Test

31. As noted above, in addition to least squares models, the *Berkman Study* estimated a mixed effects model,[23] which contains both fixed and random effects. In a mixed effects model, fixed effects are estimated directly and the random effects are calculated indirectly via an estimated variance-covariance matrix.[24] As a result, a mixed effects estimator attempts to take advantage of information contained in variation *between* countries, whereas the fixed effects estimator includes controls for *each specific country*. Both techniques allow one to estimate a regression coefficient on a variable such as *GDP* or *GUyrs*, but the two estimation techniques analyze the data differently. A benefit of the random effects model is that it has

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[23] *Berkman Study* at 139.

[24] For a more complete explanation of mixed effects models, *see* Jose C. Pinheiro and Douglas M. Bates, *Mixed-Effects Models in S and S-PLUS* (Springer 2000).

greater degrees of freedom, because the estimation of many indicator variables is unnecessary.[25] Random effects estimation, however, relies on an assumption that the random effect itself is uncorrelated with the right hand side variables in the regression, which may be unlikely.

32. The key factor in selecting between fixed effects and random effects models is whether the assumptions underlying the random effects approach holds. If the effect is uncorrelated with other right hand side variables, then random effects is consistent and efficient relative to fixed effects. If this assumption fails, however, random effects models are inconsistent, and the fixed effects approach is preferred.

33. The appropriate test for determining which approach is preferred is the Hausman Specification Test,[26] which we applied to the data used in the de Ridder and Berkman analyses. The test is constructed as follows. First, both models are estimated in accordance with the original de Ridder specification, with price on the right-hand side. Next, the coefficients and variance-covariance matrices are statistically compared between the two models. If significant differences exist, then a mixed (or random) effects approach is superior. Otherwise, the fixed effects regression is preferred. We found that the *Berkman Study's* mixed-effects model failed the Hausman Test: That is, contrary to the Berkman Study's contentions, the mixed effects model does not offer statistical efficiency above that of the fixed effects model. Moreover, when we estimated the fixed effects model using the de Ridder data, the positive effects of unbundling highlighted in the *Berkman Study* disappear.

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[25] *See, e.g.*, William H. Greene, *Econometric Analysis* 287-88 (Prentice Hall 5th ed., 2003) (introducing the fixed effects estimator) (hereafter *Greene*).

[26] *Greene* at 301-302 (discussing the Hausman Specification Test for fixed versus random effects models).

34. To perform the Hausman Test, we first estimated a fixed effects model on the following variables:[27]

- log of DSL price (lnpdsl)
- Urbanization (uurb)
- Facilities competition (Cfac)
- Years of unbundling (GUyrs)
- Indicator variable for 2005 (Dummy)

We found that the coefficient on *GUyrs* is statistically insignificant.

35. Next, we estimated both a random effects model and a mixed effects model using identical regressors to the fixed effects regression outlined above. The Hausman Test generated a Chi-Squared statistic of 3.33 when comparing random to fixed effects, and a statistic of 4.04 when comparing the mixed and fixed effects estimates. Neither of these statistics was sufficiently large to reject the null hypothesis that fixed effects is the more efficient estimator.[28] Consequently, the regression results presented in the *Berkman Study* are questionable not only because of the limited data sample and the exclusion of a relevant variable, but also because they result from use of an inappropriate regression technique.

3. The *Berkman Study* Makes Inappropriate Modifications to the Data

36. In several of its regression specifications, the *Berkman Study* changes the underlying data used by de Ridder. Specifically, as shown in Table 4.8 of the *Berkman Study*,

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[27] We include the variable names, as listed in both the *de Ridder Study* and the *Berkman Study* for convenience.

[28] This result was also robust to the use of the *Berkman Study's* modified *GUyrs* variable, in which they altered the values in 2005 for 17 of 30 countries (we discuss this in more detail below). In particular, substituting that formulation of *GUyrs* still results in a failure of both random and mixed effects relative to fixed effects. Moreover, the *GUyrs* variable in the fixed effects model is not statistically significant.

the authors altered 17 of the 30 values of *GUyrs* from the values used in the original de Ridder analysis. Using the altered data, the *Berkman Study* finds that *GUyrs* "seems to have larger effects" and "is much more significant."[29] Given the nature of the data alterations, these results are hardly surprising.

37. As the *Berkman Study* notes, its data alterations result "in many more countries defined to have *GUyrs* = 0 than before."[30] More specifically, the countries for which *GUyrs* were set to zero were Belgium, United States, United Kingdom, Luxembourg, Germany, Ireland, Poland, and Greece. The average value of *Qtot* in 2005 for these eight countries was 11.22, significantly below the average of 14.14 for all 30 countries. (The lower average for the eight "zeroed-out" countries is driven primarily by Ireland, Poland, and Greece, which have particularly low broadband penetration.) Thus, the *positive* effect of *GUyrs* in the regression specifications utilizing the altered data is the result of the exclusion of three countries that had very low broadband penetration but had adopted unbundling. In addition, the United States is assigned zero years of unbundling in 2005 despite the fact that it began to require unbundling in 1996 and, for a time, even required line sharing. And Germany is also assigned a zero, despite the fact that ECTA reported that it had 2.5 million unbundled lines devoted to broadband out of a national total of 10.7 million broadband lines![31] In other words, many of the regression results reported by the *Berkman Study* are the result of simply replacing the values for selected observations with data points that are more favorable to its conclusions. Obviously, the Commission should not base its policies on analyses of what amounts to manufactured data.

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[29] *Berkman Study* at 146.
[30] *Id.* at 145.
[31] ECTA, Broadband Scorecard, 4th quarter, 2005.

B. The Evidence from Other Quantitative Studies Does Not Support Unbundling

38. In addition to the *de Ridder Study*, there is a significant body of quantitative research into the effects of mandatory unbundling on broadband penetration. Despite its mandate to conduct a "review of existing literature and studies about broadband deployment and usage," the 231-page *Berkman Study* devotes only *half a paragraph* to reviewing this literature, briefly mentioning only five studies, two of which it concedes do not find unbundling to have a significant effect on penetration. Nevertheless, based on its highly selective literature review, the *Berkman Study* concludes that "unbundling had a positive and significant effect on levels of penetration."[32] In this section, we conduct a more complete review of the existing literature and show, contrary to the *Berkman Study's* conclusion, that the vast majority of studies find either no relationship or a negative relationship between unbundling and broadband penetration. We also note that most studies also find evidence that platform competition – i.e., the U.S. model, which is rejected by the *Berkman Study* – does have a positive and significant effect. In short, the quantitative evidence is directly contrary to the *Berkman Study's* policy recommendations.

1. Summary of Prior Empirical Studies

39. Table 4 summarizes the pre-existing empirical literature on broadband penetration, availability, and mandated unbundling. As the table shows, the bulk of the studies surveyed do not support the proposition that mandated unbundling, with its focus on intra-platform competition, increases broadband penetration or deployment. Most studies find the relationship to be either a negative or insignificant. The few studies reporting positive effects

[32] *Berkman Study* at 115.

fail to provide persuasive evidence, owing to various biases and data deficiencies. On the other hand, the majority of the studies examining the role of inter-platform competition (all but one) find that competition across platforms leads to increased broadband penetration. Thus, the existing literature suggests that mandatory unbundling is either ineffective or counterproductive in increasing broadband penetration.

Table 4:
Summary of Empirical Studies

Study	Data	Unbundling Increases Broadband Penetration/Availability?	Inter-Platform Competition Increases Penetration?
Studies Showing Negative and/or Insignificant Effects of Unbundling on Penetration			
Aron & Burnstein (2003)	Cross-Section, 46 US States	N	Y
Bauer, Kim, & Wildman (2003)	Cross-Section, 30 OECD Countries	N	N/A
Denni & Gruber (2005)	Panel, 50 U.S. States	N	Y
Distaso, Lupi & Mantenti (2005)	Panel, 14 European Countries	N*	Y
Cava-Ferreruela & Albau-Munoz (2006)	Panel, 30 OECD Countries	N	Y
Wallsten (2006)	Panel, 30 OECD Countries	N	N/A
Waverman Meschi, Reillier, and Dasgupta (2007)	Panel, 12 European Countries	N	N/A
Boyle, Howell, and Zhang (2008)	Panel, 30 OECD Countries	N	N
Wallsten and Haulsaden (2009)	Panel, 27 European Countries	N	N/A
Studies Showing Positive Effects of Unbundling on Penetration			
Garcia-Murillo (2005)	Cross-Section, 18 Countries	Y	N/A
Grosso (2006)	Panel, 30 OECD Countries	Y	Y
deRidder (2007)	Panel, 30 OECD Countries	Y	N/A

*Distaso, Lupi & Mantenti (2005) find that inter-platform competition is a substantial driver of broadband penetration, and that competition within the market for DSL services does not play a significant role. Somewhat paradoxically, the researchers also find that a decrease in the local loop unbundling price has a positive and significant effect on penetration. However, as noted above, their econometric analysis assumes that mandated access prices are exogenous, implying that this effect may reflect reverse causality.

40. In addition to the studies summarized in Table 4, there is a substantial empirical literature on the relationship between unbundling and investment. As we discuss below, the

evidence from these studies strongly supports the hypothesis that unbundling regulation reduces infrastructure investment

2. Studies Finding Negative and/or Insignificant Effects of Unbundling on Penetration

41. As shown in Table 4, we identified nine studies that find either a negative or an insignificant effect of unbundling on broadband penetration.

42. First, using a cross-section of 46 U.S. states from the year 2000, Aron and Burnstein (2003) estimate the effect of intermodal competition on broadband penetration, relative to the effect of simple broadband availability, while controlling for various demand and cost drivers.[33] The authors measure head-to-head intermodal competition as the percentage of the population in a given state residing in cities where both cable modem and DSL are deployed; broadband availability is measured by the percentage of the population of a given state residing in cities where cable modem or DSL have been deployed. The demand controls include the percentage of households with internet access and an education metric; the cost controls include a metric for teledensity (measuring the number of switched access lines per mile), the average length of a local switched access line, and the regulated price of an unbundled network element.[34]

43. The regression results reveal that, although broadband penetration is positively correlated with broadband availability, this effect disappears after controlling for intermodal competition. Thus, for a given level of demand and cost drivers, an increase in broadband availability in areas without intermodal competition does not stimulate additional adoption of

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[33] Debra Aron & David Burnstein, "Broadband Adoption in the United States: An Empirical Analysis," Working Paper, LECG Ltd. (March 2003).

[34] *Id.* at 11-12.

broadband. Furthermore, the relationship between unbundled access prices and penetration is not statistically significant.[35]

44. Analyzing a cross-section of 30 OECD countries from the year 2001, Bauer, Kim, and Wildman (2003) examine the effect of various policy variables on broadband penetration, including unbundling, cable-telco cross ownership, and government funding for broadband.[36] The researchers allocated the countries in their sample into one of three clusters, depending on the extent to which each of these policies was in place. The authors find that two factors, the population density and the "preparedness" of a given country (as captured by an index measuring attitudes towards advanced information technologies and the availability of complementary technologies, such as computers) are consistently significant in explaining broadband penetration. The analysis fails to detect any statistically significant relationship between membership in a given policy cluster and broadband penetration.[37]

45. Denni and Gruber (2005)[38] analyze biannual state-level panel data from 1999 to 2004 in an effort to determine the extent to which intra- versus inter-platform competition affects broadband penetration, using a logistic model of technology diffusion. The dependent variable in their model is the ratio of broadband subscribers to the population of a given state. Inter-platform competition is measured with a modified version of the traditional Herfindahl index, using technologies' market shares instead of firms' market shares.[39] Intra-platform

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[35] *Id.* at Table 3.

[36] Johannes Bauer, Jung Kim, & Steven Wildman, "Broadband Uptake in OECD Countries: Policy Lessons and Unexplained Patterns," Paper prepared for the European Regional Conference of the International Telecommunications Society (August 2003), at 14.

[37] *Id.* at 17-18.

[38] Mario Denni & Harald Gruber, "The Diffusion Of Broadband Telecommunications: The Role Of Competition," Paper presented at the International Communications Society Conference (2005).

[39] *Id.* at 10. For example, if half of all broadband connections were served by DSL, and the other half by cable, then the index would be computed as $0.5^2 + 0.5^2 = 0.5$

competition is measured somewhat differently, using a special case of the Herfindahl index that applies when all firms have symmetric shares.[40] Due to the potential endogeneity of these competition indices, the authors use lagged values of the endogenous variables as instruments.

46. The authors find that inter-platform completion plays a far more important role than intra-platform competition in determining the rate of diffusion of broadband infrastructure: Inter-platform competition is shown to have a substantial positive effect on diffusion in the long run, whereas intra-platform competition has only a small initial effect that rapidly dissipates. Furthermore, the authors find that mandatory unbundling actually inhibits broadband penetration. Specifically, the results indicate that the share of central offices upgraded for equal access has a negative and statistically significant effect on the rate of broadband diffusion.[41]

47. Distaso, Lupi, and Manenti (2005) develop and estimate a model of oligopolistic competition between differentiated products to analyze the relative importance of intra-platform competition and inter-platform competition in driving broadband adoption.[42] A key implication of their theoretical model is that inter-platform competition (between alternative platform providers such as cable companies and fiber-optic providers) should be more effective than intra-platform competition (competition among incumbents and DSL providers using unbundled loops) in increasing broadband penetration.[43]

48. When estimating their model, the authors employ a panel data set of 14 European countries running from the fourth quarter of 2000 through the second quarter of 2004. The

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[40] *Id.* at 11. For example, if there are three DSL providers, the competition index for that market is equal to 1/3.

[41] *Id.* at 13.

[42] Walter Distaso, Paolo Lupi, & Fabio Manenti, "Platform Competition And Broadband Uptake: Theory And Empirical Evidence From The European Union," Paper presented at the joint PURC - University of Florida and LBS 2005 telecommunications conference (April 2005).

[43] *Id.* Corollary 1, at 12.

dependent variable in each of their econometric specifications is broadband penetration, as measured by the percentage of all access lines (copper, cable, fiber, and satellite) that have been upgraded to transmit high-speed data.[44] The authors measure the degree of intra-platform competition as the level of concentration in the DSL market, using a standard Herfindahl index. The degree of inter-platform competition is modeled using a modified Herfindahl index, computed using technologies' market shares instead of firms' market shares, as in Denni and Gruber (2005). To control for potential endogeneity of the competition metrics, the two Herfindahl indices are instrumented using their lagged values.

49. Consistent with their theoretical predictions, the authors find that, although inter-platform competition is a substantial driver of broadband adoption, competition within the market for DSL services – the type of intra-platform competition that mandatory unbundling of broadband is designed to stimulate – does not play a significant role.[45]

50. Cava-Ferreruela and Alabau-Munoz (2006),[46] employing data from a panel of 30 OECD countries from 2000 to 2002, explore the determinants of wireline broadband coverage, (defined as the percentage of local loops that are DSL-enabled), as well as cable broadband coverage (defined as the percentage of homes passed by cable television networks).[47] The analysis indicates that gross national income per capita is the single most important determinant of both DSL coverage and cable coverage. The authors also find evidence that the share of

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[44] *Id.* at 13.

[45] The authors do find evidence that lower unbundling prices are significantly associated with higher levels of broadband penetration. However, their analysis assumes that the regulated price of a local loop is determined exogenously. *Id.* at 16. As discussed in more detail below, to the extent that opportunistic regulators choose to lower access prices in response to increased levels of broadband investments by incumbents, this assumption is invalid, and results in a spurious negative correlation between penetration and unbundling prices.

[46] Inmaculada Cava-Ferreruela & Antonio Alabau-Munoz, "Broadband Policy Assessment: A Cross-National Empirical Analysis," *Telecommunications Policy* 30 (2006).

[47] The authors are unable to distinguish between cable networks that have or have not been upgraded to provide broadband service. *Id.* at 449.

DSL-enabled local loops is dramatically higher in countries where inter-platform competition is more robust, whether measured by the presence of cable infrastructures or by the number of competitors operating competing broadband platforms. In contrast, neither the existence of unbundling regulations nor the number of unbundled loops is found to be significantly correlated with DSL coverage.[48]

51. Using a panel of 30 OECD countries from 1999-2003, Wallsten (2006) investigates the determinants of broadband subscribers per capita. Wallsten's regression analysis includes dummy variables for (1) various types of unbundling regulation (full unbundling, bitstream unbundling, and subloop unbundling); (2) collocation regulations; and, (3) access price regulation. The regressions also control for GDP per capita, and the number of fixed telephone lines per capita.[49]

52. In his fully specified model, which includes time and country fixed effects, Wallsten finds no consistent evidence that full unbundling has a positive effect on penetration. The effect of full unbundling is positive and significant in some specifications, negative and significant in others, and statistically insignificant in his full specifications that include all covariates. With respect to bitstream unbundling, the estimated coefficients are positive, but statistically insignificant in the full specification. Furthermore, with respect to sub-loop unbundling, the effect on penetration is consistently negative and statistically significant. Wallsten does find evidence that on-site collocation requirements are positively and

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[48] *Id.* at 455.

[49] Scott Wallsten, "Broadband and Unbundling Regulations in OECD Countries," AEI-Brookings Joint Center for Regulatory Studies, Working Paper 06-16 (June 2006).

significantly correlated with penetration. However, he also finds a negative and statistically significant relationship between regulation of collocation charges and broadband penetration.[50]

53. Waverman, Meschi, Reillier, and Dasgupta (2007) employ a panel of 12 European countries from 2002-2006 to estimate the effect of unbundling on broadband penetration.[51] The researchers' control variables include GDP, a lagged Herfindahl index (computed using technology platform shares) and the share of internet-ready cable plant.[52] Mandatory unbundling is captured with a variable measuring the price of a fully unbundled local loop as well as the number of years since unbundling was implemented. The coefficient on the unbundled price is also allowed to vary depending on whether bitstream access is available. The authors report that the number of years since adoption of unbundling has no statistically significant effect on penetration, and that the coefficients on the unbundled price variables are negative and statistically significant. This implies that that lower unbundling rates induced substitution away from alternative platforms and toward copper platforms, and that the net effect over the sample period was to steeply reduce the number of broadband consumers.[53]

54. As noted above, Boyle, Howell, and Zhang (2008) analyze a panel of 30 OECD countries from 2002 – 2005 to estimate the determinants of broadband penetration under two sets of specifications.[54] In one set of regressions, unbundling is measured with a simple

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[50] *Id.* at Table 2.

[51] Leonard Waverman, Meloria Meschi, Benoit Reillier, & Kalyan Dasgupta, "Access Regulation and Infrastructure Investment in the Telecommunications Sector: An Empirical Investigation," Working Paper, LECG Ltd. (Sept. 2007).

[52] Unlike other studies, the authors do not use the lagged Hirfindhal index to instrument for the contemporaneous index, and instead estimate an equation in which the lagged index enters directly as an independent variable. As a consequence, the authors do not directly estimate the role of inter-platform competition.

[53] *Id.* at Table 4.

[54] Glenn Boyle, Bronwyn Howell, & Wei Zhang, "Catching Up in Broadband Regressions: Does Local Loop Unbundling Really Lead to Material Increases in OECD Broadband Uptake?," New Zealand Institute For The Study Of Competition And Regulation (July 2008).

indicator variable equal to one if the country has implemented local loop unbundling in the year in question, and zero else. In a second set of specifications, unbundling is measured as the number of years since local loop unbundling was first implemented. The authors control for the retail price of DSL, the urban percentage of the population, the age of the population, and the number of non-DSL connections as a percentage of total broadband connections (to control for the presence of competing platforms).

55. The second set of regressions also includes a variable equal to the number of years for which broadband technology has been available in each country, to control for diffusion of broadband over time. (In the absence of this control variable, the unbundling variable employed in the second set of regressions might simply reflect spurious, diffusion-driven correlations). In all specifications, the authors find that the relationship between unbundling and penetration is statistically insignificant.[55] Finally, the authors do not find a statistically significant relationship between platform competition and penetration, although, unlike most researchers, they do not attempt to correct for endogeneity in the competition metric.

56. Finally, using a biannual panel of 27 European countries from 2002 to 2007, Wallsten and Haulsaden (2009) investigate the effects of unbundling on the penetration of next-generation broadband technology.[56] The authors estimate a regression in which the number of fiber broadband connections per capita is specified as a function of GDP per capita and the number of unbundled lines per capita (defined as the number of per-capita DLS connections offered over either unbundled loops or through bitstream unbundling). The authors also include

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[55] *Id.* at Table 2.

[56] Scott Wallsten and Stephanie Hausladen, "Net Neutrality, Unbundling, and their Effects on International Investment in Next-Generation Networks," *Review of Network Economics* 8(1) (March 2009).

country and time fixed effects. Estimating separate equations for incumbents and entrants, the authors find, in both cases, that the relationship between unbundling and fiber per capita is negative and statistically significant.[57]

3. Studies Finding Positive Effects of Unbundling on Penetration

57. Garcia-Murillo (2005) analyzes a cross-section of countries from 2001 to investigate the factors determining broadband availability and penetration, using two types of specifications.[58] The first set of results is of limited interest in the current context, because the dependent variable is a simple, binary indicator of whether broadband has been deployed at all in a given country. Such an analysis is incapable of assessing the degree to which unbundling does (or does not) increase penetration or investment in countries where broadband has already been deployed.

58. The dependent variable employed by Garcia-Murillo in the second set of regressions, the percentage of internet users subscribing to broadband, is more relevant. Explanatory variables in these regressions include GDP per capita, the retail price of broadband, the number of broadband providers, the percentage of internet users, and an indicator for unbundling, which is found to be positive and statistically significant in one specification. However, the sample size is quite small (less than 20), and many of the econometric results are quite anomalous, which calls the overall reliability of the model into question. For example, GDP per capita is found to have no statistically significant effect on penetration, while the

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[57] As a practical matter, the architecture of the next-generation networks that have deployed in the United States would make unbundling either extremely costly or flatly infeasible. *See* Robert Crandall, Jeffrey Eisenach, & Robert Litan, "Vertical Separation of Telecommunications Networks: Evidence from Five Countries," *Federal Communications Law Journal* (forthcoming).

[58] Martha Garcia-Murillo, "International Broadband Deployment: The Impact of Unbundling," *Communications & Strategies* 57 (2005).

estimated relationship between broadband penetration and the retail price of broadband is positive and statistically significant.[59]

59. Grosso (2006) analyzes a panel of 30 OECD countries from 2001-2004, and estimates an econometric model in which broadband penetration is a function of several variables, including a dummy variable for local loop unbundling.[60] Additional independent variables in the model include GDP per capita and lagged broadband penetration, as well as a variant of the Herfindahl index computed using technology (platform) shares to measure cross-platform competition. The econometric results indicate a negative and statistically significant relationship between the Herfindahl index and broadband penetration, indicating that cross-platform competition increases broadband penetration. In addition, the relationship between unbundling and penetration is positive and statistically significant. However, due in part to data limitations, several key explanatory variables are omitted from the analysis, including demand and cost drivers such as the price of broadband and population density.[61]

60. Importantly, both Garcia-Murillo (2005) and Grosso (2006), along with nearly all empirical researchers, assume that unbundling policies are exogenous when estimating the effect of unbundling on broadband penetration. This ignores the fact that regulatory outcomes such as mandated access prices and the adoption of unbundling policy regimes are subject to political and administrative processes, which implies that they are endogenous.[62] Endogeneity

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[59] *Id.* at Table 8.

[60] Marcelo Grosso, "Determinants of Broadband Penetration in OECD Nations," Working Paper, Regulatory Development Branch, Australian Competition and Consumer Commission (2006).

[61] *Id.* at Table 1.

[62] See, e.g., J. Gregory Sidak and Daniel F. Spulber, "Deregulatory Takings and Breach of the Regulatory Contract," *New York University Law Review* 71(4) (1996), discussing circumstances under which mandatory unbundling can lead to "deregulatory takings" by opportunistic regulatory agencies; *see also* David Newbery, *Privatization, Restructuring, and Regulation of Network Utilities* (MIT Press 2002); *see also* Jeffrey A. Eisenach and Hal J. Singer, "Irrational Expectations: Can a Regulator Credibly Commit to Removing an Unbundling

bias is driven by the fact that regulators may respond to incumbents' infrastructure investments by providing easier access to entrants (by mandating unbundling and/or lowering access prices). Thus, unbundling may appear to drive increased investment and broadband penetration, when in fact the causation runs in the opposite direction. Only a handful of studies have attempted to assess the empirical magnitude of this source of bias. Those that have find substantial evidence of endogenous regulation.[63]

61. Moreover, as we discussed above in the context of the *de Ridder Study*, few empirical studies control for the fact that the passage of time should have a substantial effect on penetration, due to technology diffusion. So-called general purpose technologies, such as broadband, tend to follow a well-known "S-shaped" curve as they mature.[64] While initially only early adopters find it worthwhile to purchase the technology, eventually the technology achieves mass-market acceptance and adoption accelerates. Finally, the rate of adoption begins to level off as saturation approaches, and the few remaining non-adopters tend to be those who place a relatively low value on the technology. As noted above, Boyle et. al. (2008) have shown that failure to control for diffusion over time can lead to spurious correlations between unbundling and penetration. Furthermore, this effect will be exacerbated by endogenous

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Obligation?" AEI-Brookings Joint Center Working Paper No. 07-28 (December 2007), available at: http://ssrn.com/abstract=106516.

[63] *See, e.g.*, Robert Crandall, *Competition and Chaos* (Brookings Institution Press 2005), at 71, providing evidence that regulators lower access prices in response to investments by incumbents; *see also* Tomaso Duso & Lars-Hendrik Röller, "Endogenous Deregulation: Evidence from OECD Countries," *Economic Letters* 81(1) (2003), showing that political indicators explain the degree of deregulation in the mobile telecommunications industry; *see also* Michal Grajek and Lars-Hendrik Röller, "Regulation and Investment in Network Industries: Evidence from European Telecoms," ESMT Working Paper No. 09-004 (2009). Using political and geographic variables as instruments, as well as lagged endogenous variables, Grajek and Röller provide empirical evidence that access regulation is determined endogenously, and that failure to control for this bias distorts the statistical relationship between regulation and telecommunications investment decisions.

[64] *See, e.g.*, Elhanan Helpman, ed., *General Purpose Technologies and Economic Growth* (MIT Press 1998).

regulatory outcomes, to the extent that regulatory commitment problems become more acute with movements along the diffusion curve, as broadband adoption – and the infrastructure investments that make it possible – begin to accelerate.

4. Studies Relating Unbundling to Network Investment

62. There is also a large body of empirical work focusing on the relationship between mandatory unbundling and network investment, which has demonstrated that mandatory unbundling discourages investment by both incumbents and entrants[65] and thus calls into question the "stepping stone" hypothesis,[66] which posits that mandatory unbundling can create a set of "rungs" on a "ladder of investment" allowing entrants to invest gradually in their own facilities. Indeed, the primary author of the ladder of investment thesis, Dr. Martin Cave, has acknowledged that it "remains no more than a hypothesis, as scientific testing of an imprecise proposition of this kind remains problematic."[67] The most recent authoritative review of the literature on unbundling and investment examines more than 20 empirical studies of access regulation and investment incentives, and concludes that while additional research could be useful, "most of the evidence shows that local loop unbundling…discourages both ILECs

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[65] *See, e.g.*, Robert W. Crandall, Allan T. Ingraham and Hal J. Singer, "Do Unbundling Policies Discourage CLEC Facilities-Based Investment?" *Topics in Economic Analysis and Policy* 4 (2004). The authors use cross-state and within-state variation in the price of constructing local phone lines relative to leasing unbundled loops to identify the sensitivity of CLEC investment in local lines to the LLU rate. They show that mandatory unbundling encourages a CLEC to delay facilities-based investment by altering its relative net present value of investment between time periods. See also Jeffrey A. Eisenach, Paul Lowengrub and James C. Miller III, "An Event Analysis Study of the Economic Implications of the FCC's UNE Decision: Backdrop For Current Network Sharing Proposals," *Commlaw Conspectus* 17;1 (2008). An even larger body of research has examined competition and liberalization in regulated industries more generally. See Mark Armstrong and David Sappington, "Regulation, Competition, and Liberalization," *Journal of Economic Literature* 44:2 (2006

[66] Martin Cave, "Encouraging Infrastructure via the Ladder of Investment," *Telecommunications Policy* 30 (2006).

[67] Martin Cave, "Applying the Ladder of Investment in Australia," (December 17, 2007), at 1.

and CLECs from investing in networks."[68] The summary table from that study is reproduced as Exhibit E to this Declaration.

63. The *Berkman Study* does not express disagreement with the above results. Rather, it simply ignores the effect of policy on private network investment. Instead it focuses approvingly on public investment in broadband network infrastructure. It should be noted that many countries whose regulatory policies have failed to induce investment by incumbents or entrants are now turning to public investment to remedy their policy errors.[69] We doubt that U.S. authorities would welcome such an outcome.

IV. THE *BERKMAN STUDY'S* CONCLUSIONS ARE NOT SUPPORTED BY QUALITATIVE ANALYSIS

64. To advance its case for an expanded network unbundling regime in the United States, the *Berkman Study* attempts to buttress its quantitative analysis with extensive discussions of unbundling policies in various OECD countries. While it does not suggest that every country that has pursued unbundling has achieved success through such a policy, its general conclusion is that unbundling has generally had favorable effects. As we explain below, like the quantitative evidence, the qualitative evidence does not support the *Berkman Study's* conclusions.

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[68] Carlo Cambini and Yanyan Jiang, "Broadband Investment and Regulation: A Literature Review," *Telecommunications Policy* (2009) (in press), at 11-14. (Although the article's title might suggest an exclusive focus on broadband, in reality the authors provide an extensive survey of the literature examining the relationship between regulation and investment in telecommunications infrastructure generally). The International Telecommunications Union (ITU) echoed this finding in a recent report, noting that "the reason that there was so little investment in (local loop) infrastructure by new entrants [in the United States] was that they could not deploy new infrastructure at the regulated local service prices, which were too low and acted as a disincentive to investment. *" See* International Telecommunications Union, *Trends in Regulatory Reform 2008* (November 2008), at 52. Note also that Armstrong and Sappington, *supra*, at 360, observe that "providing entrants with long-term subsidized access to the incumbent's infrastructure...generally [is] not recommended."

[69] Two of the authors of this Declaration, Crandall and Eisenach, discuss this phenomenon in a separate paper. *See* Robert W. Crandall, Jeffrey Eisenach, and Robert Litan. (2009), "Vertical Separation of Telecommunications Networks: Evidence from Five Countries," *Federal Communications Law Journal* (forthcoming) (available at http://ssrn.com/abstract=1471960) (hereafter *Crandall, Eisenach and Litan*).

A. The Alleged Successes

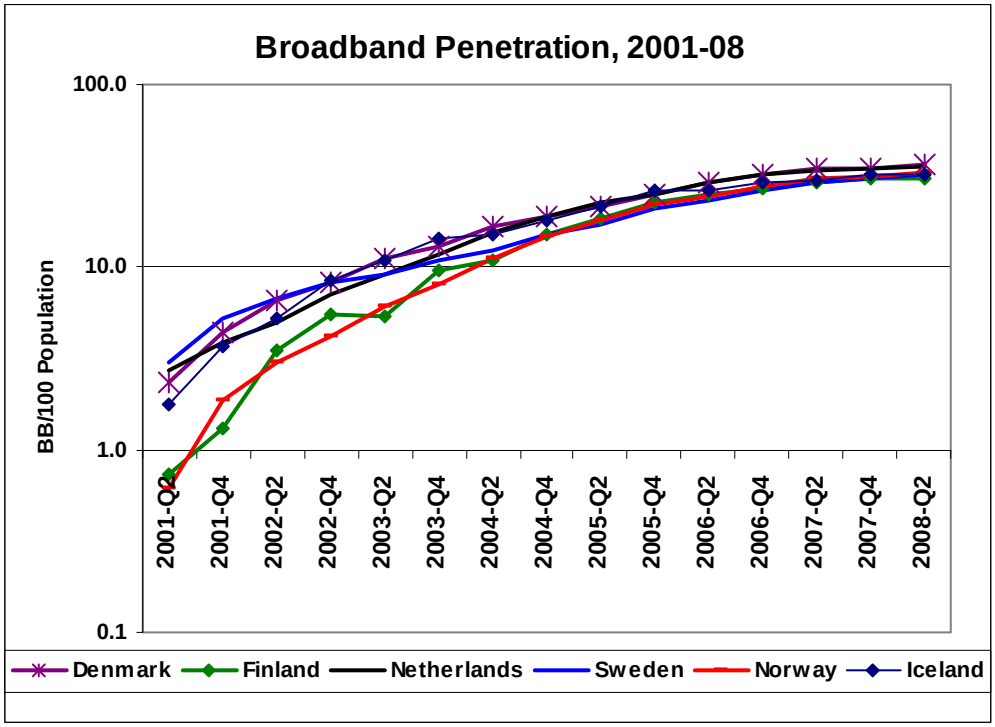
65. The *Berkman Study* concludes that network unbundling has generated competition, lower prices, and greater broadband penetration in several Nordic countries, the Netherlands, France, the United Kingdom, and Japan. A careful examination of available data for each of these countries suggests that the *Berkman Study's* conclusions are either incorrect or, at best, vastly overstated.

1. The Nordic Countries and Netherlands

66. A cursory look at the OECD broadband penetration rankings reveals that a large share of the best performers are cold, northern countries – Denmark, Norway, Sweden, Finland, Netherlands, and Iceland occupy the top six positions. Countries with more moderate climates, such as Italy, Spain, New Zealand, and Australia are far down the OECD list. Equally important, despite substantial differences in policy across these Nordic countries, all are converging to a very similar level of broadband penetration as Figure 1 shows.[70] This might suggest that cultural, weather, or other demand factors explain a large share of the variance in penetration, but the *Berkman Study* chooses to focus on network access policies. Its discussion of even the "best" performers is not persuasive.

[70] We show the growth in broadband penetration on a logarithmic scale in this and the next figure to illustrate the *rate* of growth of broadband over time in these two countries.

Figure 1



Source: OECD

67. In discussing the Nordic countries, the *Berkman Study* is somewhat equivocal about the effects of network unbundling. With respect to Denmark, for example, it simply concludes that

> Competition developed both within each technological platform, and across platforms; to some extent benefiting from unbundling when it was available, and to some extent benefiting from relatively low levels of investment necessary to upgrade an existing infrastructure like cable….[71]

The report does not point out to the reader that cable modems exceed the number of unbundled and shared lines in Denmark,[72] or that cable modem competition in Denmark was highly developed before the recent growth of unbundled lines to deliver DSL.[73] Thus, intra-platform competition appears to have *followed* inter-platform competition in Denmark, and the available

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[71] *Berkman Study* at 91.
[72] ECTA Broadband Scorecard, 2009, 1st Quarter.
[73] ECTA Broadband Scorecards, various editions.

empirical evidence suggests that such inter-platform competition is far more important than intra-platform competition.

68. In Sweden, platform competition has also been responsible for more broadband subscriptions than has network unbundling. As of the first quarter of 2009, cable connections and non-incumbent telco fiber to the home, provided largely over government fiber networks, accounted for 1.2 million connections while DSL over unbundled and shared loops for only 600,000 connections, and the number of lines delivered over unbundled/shared lines is actually declining.[74] Much of Sweden's "success' derives from its government-provided fiber, not from network unbundling. Nevertheless, the *Berkman Study* relies on the views of the Swedish regulator:

> Convinced by the perceived success of unbundling in fostering competition, investment, and innovation in its broadband markets, concerned about managing the transition to next generation networks, and possibly smarting from the long fought battle over bitstream access, the Swedish regulator PTS concluded that it would best manage the transition to next generation connectivity by imposing functional separation on its incumbent.[75]

69. This is a curious conclusion, given that the incumbent has not been among the leaders in the EU in network investment and only has 12 percent of the country's fiber connections. The transition to next generation networks in Sweden has been over networks built largely by municipal governments, not by the incumbent operator.

70. The *Berkman Study's* dismissal of the effect of unbundling in Finland is similarly curious. It asserts that "Finland was the first Nordic country to introduce unbundling, in 1996. Unbundling seems to have had little or no effect in the Finnish market, however."[76] But

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[74] ECTA Broadband Scorecard, 2009 1st Quarter, and earlier editions.
[75] *Berkman Study* at 92.
[76] *Berkman Study* at 93.

according to latest ECTA data, broadband delivered over unbundled and shared loops accounted for 25 percent of all broadband lines while they accounted for only 18 percent in Sweden, a country that the *Berkman Study* argues is a "perceived success of unbundling." Notably, Finland – an early adopter of network unbundling – has not deployed any fiber to the home according to the ECTA data.

71. Finally, in the case of Netherlands, the *Berkman Study* suggests that network unbundling plays a critical role in allowing facilities-based entrants, the cable companies, to address customers in areas where they lack facilities. But unbundled and shared lines accounted for only about 680,000 lines in March 2009 while broadband over cable accounted for 2.2 million lines according to the latest ECTA data. And despite the *Berkman Study's* glowing description of the incumbent's (KPN's) decision to offer unbundled fiber to its competitors, the latest ECTA data show no such unbundling. Perhaps more important, the ECTA data show that *entrants* have 140,000 subscribers over their own fiber, but KPN has none, and the latest data from IDATE show that KPN has yet to even *deploy* FTTH.[77]

2. France

72. The *Berkman Study* makes much of differences between France's and Germany's unbundling policies. It paints the French policy since 2002 in very rosy terms, pointing out that an EC infringement action forced the French regulator to mandate lower rates for unbundled and shared loops. Subsequently, in 2003 new entrants expanded rapidly. By contrast, the

[77] ECTA, Broadband Scorecard, 1st quarter 2009; IDATE, *FTTH European Panorama* (February 11, 2009) at10.

German regulator is described as unwilling to enforce an unbundling regime that works.[78] These differences in policy lead the *Berkman Study* to opine that: "Despite having roughly similar GDP per capita (Germany slightly higher) and population concentration (France slightly higher), the two countries present quite different trajectories."[79] What trajectories is it referencing? A quick look at Figure 2 shows that the trajectory of broadband penetration since 2003 is very similar in the two countries. Why worry about differences in unbundling policy, given this obvious fact? Figure 2 shows that Germany actually experienced slightly greater broadband growth after 2003, a result that is largely due to the greater growth of inter-platform competition from cable television. Germany now has about 2 million cable broadband subscribers – more than twice the number in France.[80]

73. Moreover, both France and Germany remain far below the Nordic countries and Netherlands in terms of broadband penetration. Neither has yet to deploy much fiber to the subscriber. France had only 40,000 fiber loops in the first quarter of this year, out of a total of more than 18 million broadband connections; Germany had none. By contrast, Verizon alone had 3.3 million FTTH broadband connections at the end of the third quarter of 2009. Thus, while unbundling has created competition among suppliers of DSL in France, it has not led entrants or incumbents to invest much in fiber to the subscriber. Despite the *Berkman Study's* assertion that France Telecom has responded with "higher investment," its capital spending as a

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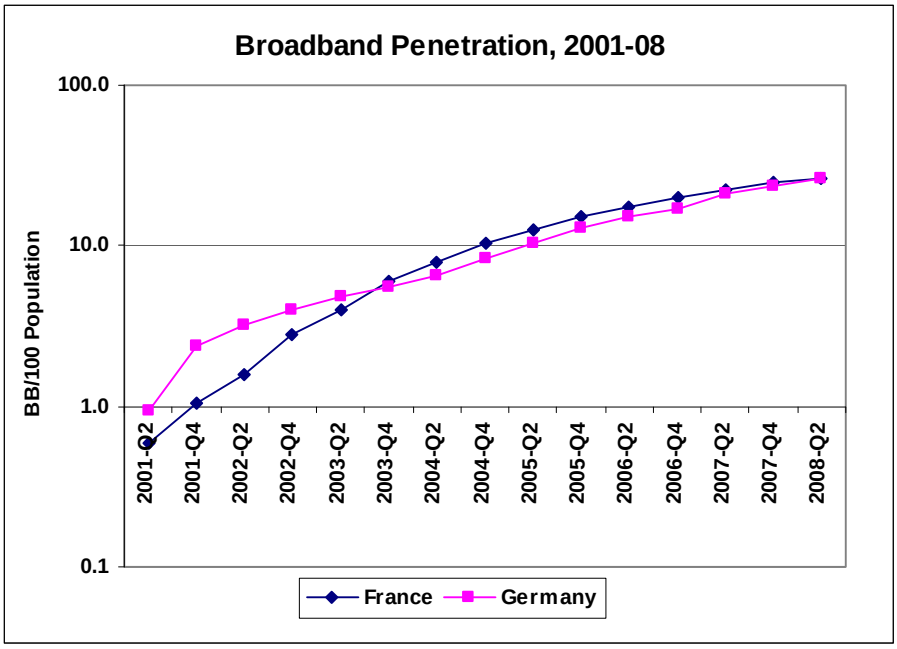
[78] It is also important to recognize that neither country has had aggressive competition from cable systems until recently. Cable's share of broadband is about 5 percent in France and 9 percent in Germany according to the most recent ECTA data.

[79] *Berkman Study* at 95.

[80] ECTA, Broadband Scorecard, 1st quarter 2009.

share of revenues remains mired in the very lowest echelons of capital spending among EU-15 incumbents.[81]

Figure 2



3. The United Kingdom

74. Two of the authors of this declaration were co-authors of an earlier study of network separation policies that is being published this year in the *Federal Communications Law Journal*. In that article, we reviewed in great depth the results of Ofcom's new (2005) unbundling and functional separation policy. We concluded that under this policy the rate of growth of the UK's broadband subscribers had declined relative to the growth in the overall EU-15.

75. Specifically, we showed that:

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[81] Company annual reports for 2008. Among the 12 incumbents for which we have data for 2008, only Belgacom has lower capital spending as a share of revenue (13.1% versus 13.2% for FT). By contrast, DT's 2008 CapEx was 14.1% of revenues.

> A comparison of UK broadband growth with growth in the EU-15 yields a similarly bleak conclusion about the effects of functional separation. According to ECTA data, between September 2002 and September 2005, when the new Ofcom policy went into effect, UK broadband lines increased at an annual rate of 76 percent while EU-15 broadband lines rose at a rate of 54 percent. Thus, prior to the change in policy, the rate of increase in UK broadband lines was 41 percent greater than the rate of increase in the EU-15. In the three years following the implementation of the new Ofcom policy, UK broadband line growth fell to 21 percent, and EU-15 broadband line growth fell to 23 percent.[82] …Thus, the new policy has been associated with a severe decline in UK growth relative to the growth in the EU-15. Indeed, the UK broadband growth rate is now less than the average rate for the entire EU-15, and broadband penetration in the UK has fallen relative to EU-15 penetration in the three years that the policy has been in place.[83]

76. The recent ECTA data do not alter this conclusion; UK broadband subscriptions have grown more slowly than EU-15 subscriptions over the 3.25 years since the new unbundling regime went into effect in the third quarter of 2005. Nor do the ECTA data show any fiber deployment in the United Kingdom through the first quarter of this year. The policy has simply not worked to accelerate the growth of broadband or encourage deployment of a Next Generation Network.

4. Japan

77. The authors of the *Berkman Study* would have us believe that Japan's policy of network unbundling has been an unqualified success, although it admits that facilities-based competition is driving fiber deployment. It claims that the Japanese regulator's decision to require fiber unbundling has not deterred fiber investment: "…the overall level of investment in the fiber market questions the argument that open access deters investment."[84] However, we can find no evidence that NTT has actually delivered a significant number – or, for that matter,

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[82] The most recent ECTA data are for the first quarter of 2009.
[83] Crandall, Eisenach and Litan at 21-22.
[84] *Berkman Study* at 85.

any number – of unbundled fiber connections. Fiber "unbundling" has not deterred NTT's investment in FTTH for a simple reason: fiber unbundling has not been implemented in a meaningful manner.

78. Contrary to the impression created by the *Berkman Study*, Softbank (Yahoo!BB) states in its most recent annual report that it is losing broadband DSL customers steadily, and it reports no customers over NTT's fiber plant. Indeed, the Softbank 2009 annual report downplays any discussion of the decline in its wireline broadband business, simply stating that it is reducing capital expenditures in its fixed-wire operations: ". . . the segment's profitability and cash flow are showing marked increases. This reflects a lower depreciation *from the end of major capital expenditures*, combined with a structural transformation toward businesses."[85] It is not investing in fiber to the home or any other form of residential connections, but rather shifting its attention to the business market.

79. In short, the unbundling era in Japan appears to be fading rapidly. DSL subscriptions have been declining for three years. Fiber connections, supplied by NTT and a host of other platform-based competitors now substantially exceed DSL connections. The principal competitors of NTT in delivering broadband over fiber optics are the country's power companies, not the companies who have used NTT's copper loops to deliver DSL service.[86] At this point, these power companies are deploying fiber without any apparent concern that they will have to unbundle their fiber networks. Platform competition, not network unbundling, is driving fiber deployment in Japan.

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[85] Softbank, *Annual Report 2009*, p. 41.

[86] The Ministry of Internal Affairs and Communications (MIC) reported in March 2009 that the power companies, including the company bought by KDDI, had nearly 16 percent of fiber connections, NTT had approximately 74 percent, and USEN, the largest cable company, had 4 percent. " *See* MIC, Disclosure of Quarterly Data Concerning Competition Review in the Telecommunications Business Field," March 25, 2009.

B. The Doubtful Cases or Outright Regulatory "Failures"

80. The *Berkman Study* reviews the regulatory developments in several OECD countries, citing their failure to embrace network unbundling, or the failure to implement it assertively, as a regulatory mistake. These countries include South Korea, Germany (already discussed), Italy, New Zealand, and Canada.

1. South Korea

81. No matter how the *Berkman Study* tries to characterize it, the South Korean broadband policy is a triumph for platform-based competition. The Study argues that:

> The South Korean experience is more *ambiguous* on access, pointing more toward heavy government investment. Both (Japan and South Korea) suggest that a strong, professional regulator, exercising effective power over incumbent providers, can foster significant market development and competition.[87]

82. But this "strong" regulation essentially amounted to subsidizing capital investments by the cable and electric utility companies, but discouraging these companies from offering retail broadband Internet services over their own platforms. As a result, new entrants sprang up offering broadband services over their own platforms and over the cable companies' and electric companies' networks.

83. As the *Berkman Study* correctly observes, this platform competition led to rapid growth in Korean broadband before any unbundling requirements were imposed on the incumbent telco, KT, in 2002. By that time, Korea had by far the highest broadband penetration in the OECD –21.8 percent versus an average of 4.8 percent in the OECD. Since 2002, Korea's penetration has grown very slowly, expanding by less than 50 percent to 32 percent in December 2008 while the average OECD country has nearly quintupled its penetration to 22.4

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[87] *Berkman Study* at 83 (emphasis added).

percent. Four Nordic countries and the Netherlands have surpassed Korea since its new unbundling requirement took effect in 2002.[88] Thus, unbundling had nothing to do with South Korea's spectacular early broadband growth.

2. Italy

84. The Italian regulator required network unbundling in 2001 and then required functional separation of Telecom Italia in 2006-08. As of March 2009, entrants using unbundled or shared lines accounted for 27 percent of all lines. The principal problem in Italy had been the absence of platform competition since it had virtually no cable television. Platform competition did emerge in the form of Fastweb's[89] fiber deployment in large cities, but this deployment has now been halted. Fastweb has chosen to grow only through leasing Telecom Italia's loops now that functional separation of TI assures them of rapid access to TI's loops as TI's own retail division.[90] Thus, contrary to the *Berkman Study*'s assertion that Fastweb will continue to deploy fiber and share it with TI, Fastweb has chosen to abandon further fiber investment because of recently improved access to TI's unbundled loops. Fastweb's recent decision is evidence of the adverse effect of mandated network sharing on investment in new technologies. But Italy's relatively low broadband penetration probably has little to do with its regulatory policy. Rather, it is likely a reflection of low *personal computer* penetration in the country.[91]

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[88] OECD Broadband Statistics, various years.

[89] Fastweb is an Italian company now owned by Swisscom.

[90] Meeting of Robert Crandall with Fastweb, Milan, Italy, September 23, 2009.

[91] Italy ranked 13th among EU-15 countries in household personal computer penetration in 2005. Fewer than 50 percent of Italian households had a personal computer. By contrast, nearly 80 percent of households in Nordic countries have a PC. *See* OECD, *2007 Communications Outlook* at 134.

3. New Zealand

85. New Zealand had decided not to mandate network unbundling in 2003, but the Communications Ministry changed course in 2006. The government imposed network unbundling and structural separation of Telecom New Zealand in December 2006, undoubtedly because broadband penetration was relatively low, and there was very little platform competition. Since that time, New Zealand's broadband penetration has grown more rapidly than the OECD average, but New Zealand's penetration is still below the OECD average. Moreover, there is no sign of fiber to the premises deployment by Telecom New Zealand or its competitors.

4. Canada

86. Canada's broadband policy has been similar to that in the United States. Canada has very strong platform competition from cable, a fact that propelled it to second place in OECD broadband penetration in 2001. As in the United States, cable broadband connections have exceeded DSL connections from the very beginning. Thus, inter-platform competition was extremely strong in Canada from the outset. Canada has had network unbundling regulations in place since 1997, but few entrants have succeeded in using unbundled loops for delivering broadband or other telecommunications services to residential customers perhaps because of the strength of inter-platform competition in most regional Canadian markets. Most of the local entrants using unbundled loops were unable to compete in this environment and either failed or were acquired. The *Berkman Study* is incorrect, however, when it asserts that "[t]here are no smaller entrants of note."[92] MTS-Allstream has been using Bell Canada's and Telus's local

[92] *Berkman Study* at 110.

loops for years, but with little apparent effect on broadband penetration because its principal focus has been the business market, not the residential market. Thus, while unbundled loops have been available in Canada for more than a decade, they have not been used successfully by entrants to compete in the mass market for broadband services.

C. Conclusions

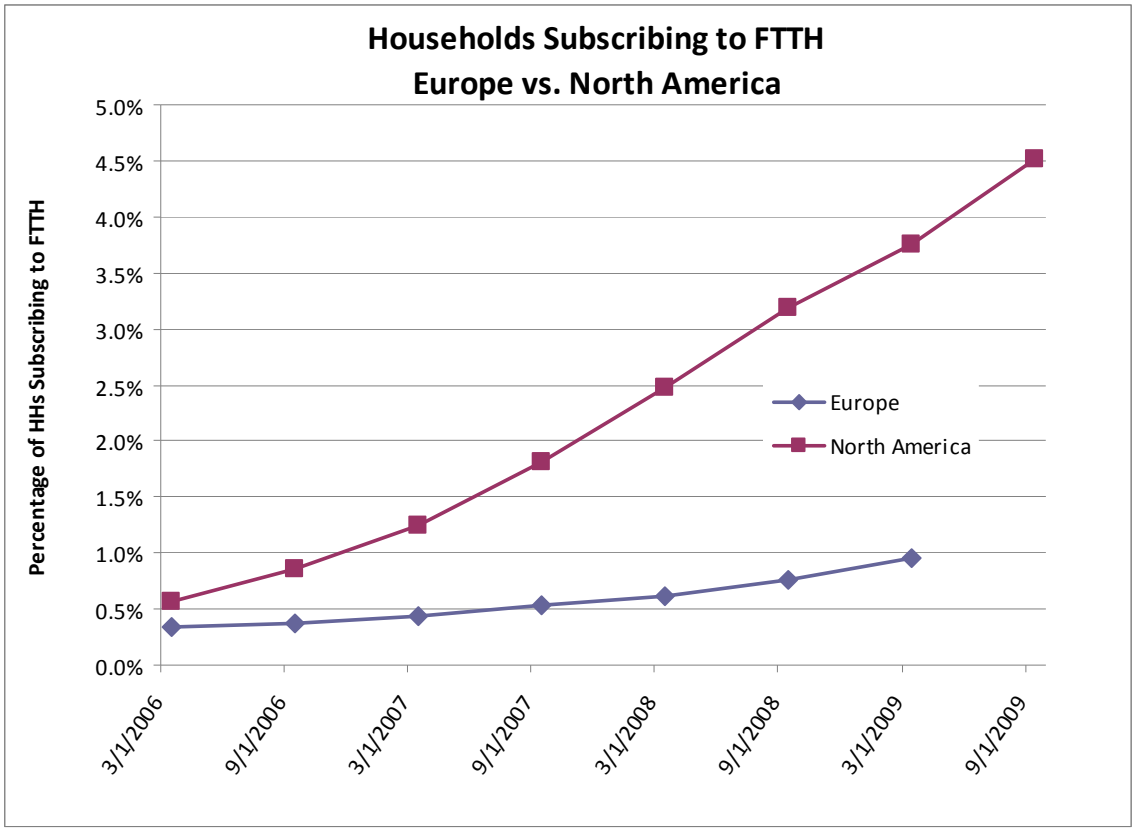
87. The *Berkman Study* has failed to unearth any systematic anecdotal information that mandated network unbundling has increased broadband penetration in OECD countries. This is hardly surprising given the preponderance of empirical studies which conclude that unbundling has limited or no effect. Its narrative does often suggest, however, that platform competition is important in driving competition in countries such as South Korea, the Nordic countries, Canada, and the Netherlands. This too is consistent with the published empirical evidence.

88. The *Berkman Study's* focus on unbundling of copper loops to promote intra-platform competition is misplaced. The issue for U.S. regulators today is not how to increase competition in DSL services, but rather how to encourage the development of new, faster networks. Even if unbundling copper loops had been a mild success in promoting DSL, this result would not suggest that a similar policy would be successful in promoting the development of new fiber-based networks. Indeed, we have shown that the empirical literature on network investment and access regulation shows that mandated access regulation reduces the incentive to invest in new network technologies.

89. The *Berkman Study* is critical of the United States and Canadian policies for not being sufficiently aggressive in promoting network sharing and is correspondingly approving of a general European regulatory bias towards encouraging such sharing. Unfortunately for the

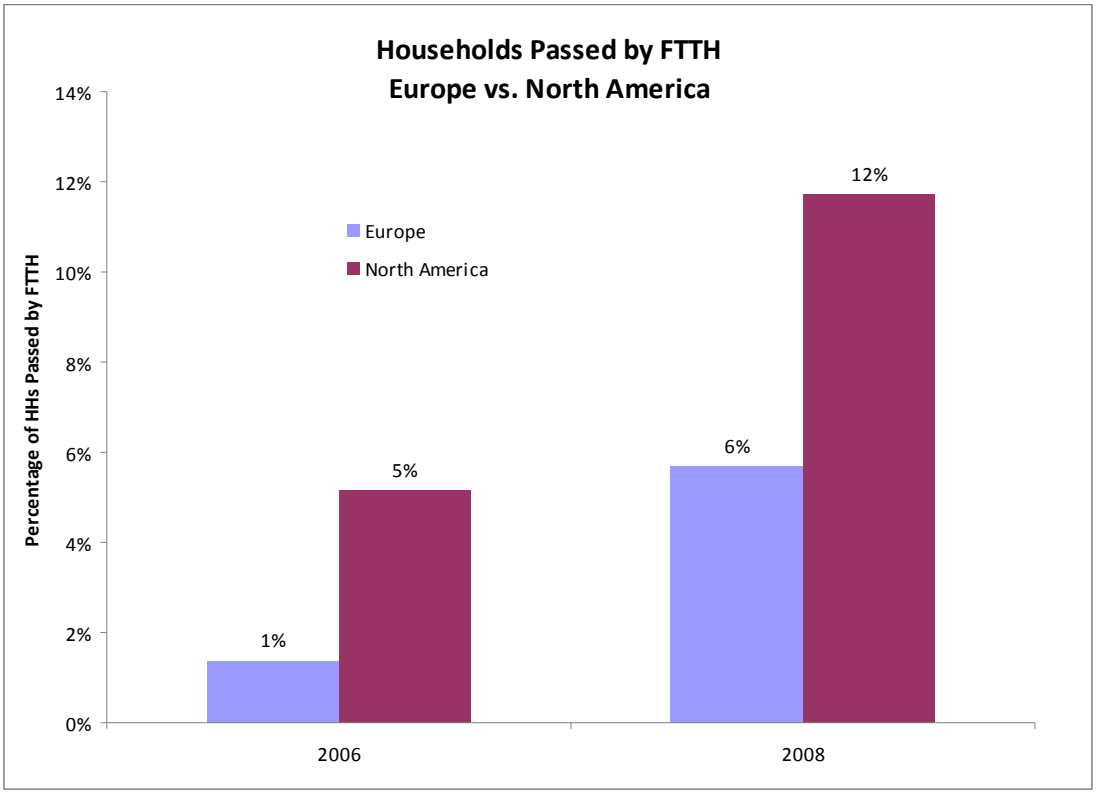
Europeans, their more aggressive regulatory policy has resulted in far less deployment of fiber in the European Union than in North America as demonstrated by Figures 3 and 4.

Figure 3



Sources: Canadian Households from www40.statcan.ca/l01/cst01/famil55a-eng.htm; U.S. Households, from http://quickfacts.census.gov/qfd/states/00000.html; European Households, from http://w3.unece.org/pxweb/Dialog/varval.asp?ma=08_GEFHPrivHouse_r&ti=Private+households+by+Household+Type%2C+Measurement%2C+Country+and+Year&path=../DATABASE/Stat/30-GE/02-Families_households/&lang=1; FTTH subscribers Europe, from ECTA; FTTH subscribers North America, from "North American FTTH/FTTP Status," Fiber-to-the-Home Council: North America (2009) at 4.

Figure 4



Sources: Household data, same as Figure 3; FTTH Homes Passed North America, from "North American FTTH/FTTP Status," Fiber-to-the-Home Council: North America (2009) at 2; FTTH Homes Passed Europe 2006 from "FTTH Situation in Europe," Idate Consulting & Research (February 2007 at 20; FTTH Homes Passed Europe 2008 from "FTTH European Panorama," Idate Consulting & Research (December 2008) at 10.

V. THE *BERKMAN STUDY'S* ANALYSIS IS FLAWED IN OTHER SIGNIFICANT RESPECTS

90. In addition to the errors noted above, the *Berkman Study* relies on erroneous analyses or makes misleading assertions on a number of fronts. In the sections below, we focus on four of these: (A) its over-reliance on bivariate analysis and "scatter plots" to support its conclusions; (B) its mischaracterization of the evolution of U.S. broadband policy; (C) its mischaracterization of the policy consensus abroad, especially in the European Union.

A. The *Berkman Study* Relies Almost Exclusively on Bivariate Analysis

91. Bivariate analysis – the comparison of two sets of data to determine the extent to which one variable is correlated with another – can be a useful means of describing the relationships between variables, and as a starting point for more rigorous multivariate (i.e., regression) analysis. It cannot, however, take the place of multivariate analysis, and can almost never support valid findings of causality. Yet, the *Berkman Study* relies heavily on such analysis to support its conclusions, presenting scatter plot after scatter plot, depicting everything from the relationship between broadband penetration and entrepreneurship (Figure 2.3) to the relationship between average download speeds and median download speeds (Figure 3.191-i). Identifying and critiquing each and every instance in which the *Berkman Study* relies inappropriately (explicitly or implicitly) on such comparisons to support its conclusions would require far more space than we can justify here.

92. We do, however, point out one particularly egregious (and significant) example of how bivariate analysis can be used to present misleading information: Figure 4.2 of the *Berkman Study* (reproduced below) presents a scatter plot which shows the results of one of the two quantitative analyses contained in the study, a survey of broadband price and speed offerings by 59 companies in OECD countries. The Berkman Study relies heavily on this analysis for its conclusions, stating that the figure

> shows that prices and speeds at the highest tiers of service follow a clear pattern. The highest prices for the lowest speeds are overwhelmingly offered by firms in the United States and Canada, all of which inhabit markets structured around "inter-modal" competition – that is, competition between one incumbent owning a telephone system, and one incumbent owning a cable system.[93]

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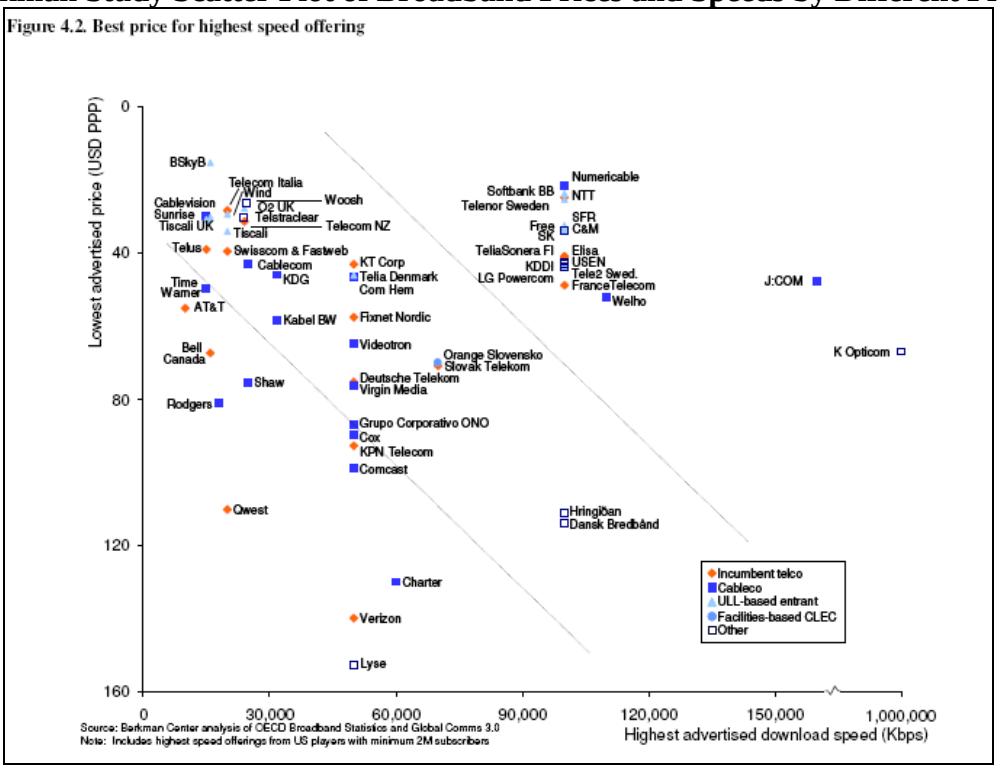
[93] *Berkman Study* at 12.

Moreover, subsequent to the study's release, author Yochai Benkler (in the course of responding to various criticisms of the study) posted a blog entry in which he stated that Figure 4.2 shows results that "are consistent with the open access hypothesis, and inconsistent with the inter-modal competition hypothesis," and that it supports "the basic storyline," that "Where a regulator rolled up its sleeves and really implemented open access, new and innovative entrants used the opportunity to invest in new service models and new electronics, introduced bundled voice over IP, or IPTV, or nomadic access."[94]

Figure 5
Berkman Study Scatter Plot of Broadband Prices and Speeds by Different Providers



93. While the *Berkman Study's* Figure 4.2 may appear visually to support the Berkman Study's "basic story line," the figure hardly constitutes "analysis" and provides

[94] See http://cyber.law.harvard.edu/node/5751.

virtually no basis for any conclusions about the relationship between unbundling and investment. For example:

1. Most obviously, the figure fails to account for other variables that likely affect the cost (and hence price) of broadband services, such as loop lengths, urbanicity, dwelling type (multi-dwelling units vs. single-family homes), and topology;

2. By the same token, the figure fails to account for policy variables other than unbundling (e.g., government subsidies) which likely affect both costs and prices;

3. The data presented shows only each carrier's "highest speed offering," which may or may not be utilized by a significant number of consumers;[95]

4. The figure compares data points for firms (such as Verizon and Qwest) that serve large, low-density areas with firms (such as Fastweb) that serve only dense urban areas.

94. Thus, even leaving aside issues of measurement (e.g, whether advertised speeds faithfully reflect delivered speeds), the "analysis" in the *Berkman Study's* Figure 4.2 is in fact nothing more than a description of the relationship between two sets of variables, from which no conclusions about causality (let alone policy) can appropriately be drawn.

B. The *Berkman Study* Mischaracterizes the Evolution of U.S. Broadband Policy

95. The *Berkman Study* is being offered at a time when the Commission and the National Telecommunications and Information Administration (NTIA) are developing a broadband plan. At the same time, the FCC is considering new rules governing "network neutrality" on broadband networks. We are not aware, however, that the FCC is planning to revisit the rules that it has established to implement the network access requirements in the 1996

[95] The *Berkman Study* acknowledges that the U.S. has among the lowest "entry level" prices, and that this fact should lead to higher levels of penetration. It never reconciles this fact with its contention that penetration in the U.S. is low as a result of the absence of unbundling, which could only increase penetration to the extent it reduces prices.

Telecommunications Act. Yet, the *Berkman Study* is openly critical of the evolution of these rules, seeing them as a reflection of incumbent "resistance" and "skeptical" courts.[96] In the course of its criticism, the *Berkman Study* mischaracterizes the evolution of U.S. policy towards unbundling.

96. At one point, the United States had the most ambitious and aggressive unbundling policy among the OECD countries. In implementing the 1996 Act, the FCC decided to require incumbents to unbundle *every* network element, not just the "local loop" that connects the subscriber to the carrier's wire center. Indeed, the incumbents were required to offer all of these elements as an "unbundled network element platform" or UNE-P at regulated rates based on forward-looking costs. No other country required such a wholesale offering. In addition, the FCC required the incumbents to share their subscriber loops with entrants seeking to offer only broadband services through the higher frequencies on the loop. The incumbents would be left with the lower frequencies to offer traditional telephone service.

97. This aggressive unbundling policy was contested in the federal courts as inconsistent with the requirements of the 1996 Act. The DC Circuit Court of Appeals and the Supreme Court forced the FCC to revise its unbundling rules in several ways. First, the Supreme Court overturned the FCC's approach to unbundling as overly broad because the Commission had failed to consider whether an entrant could self-provision the various unbundled elements or acquire them from a third party and because the Commission had

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[96] *Berkman Study* at 78. The *Berkman Study's* highly opinionated characterization of U.S. policy during the first decade of the 21st century is consistent with the previously expressed views of its principal investigator, Dr. Yochai Benkler, who (for example) has previously characterized the FCC's decision to classify cable modem service as an "information service" (a decision upheld by the Supreme Court in the *Brand X* case) as a "legally admissible policy error." See, Yochai Benkler, *The Wealth of Networks* (2006) at 400 (available at www.benkler.org/Benkler_Wealth_Of_Networks.pdf).

considered *any* increase in costs a source of "impairment" that, in turn, required access to unbundled elements at regulated prices.[97]

98. Second, the DC Circuit reversed the FCC's rules on line sharing in *United States Telecommunications Association v. FCC*, 290 F.3d 415 (2002) because the Commission had failed to evaluate the effects of inter-platform competition. The FCC complied in 2003 by dropping the line-sharing requirement. However, entrants would still be able to lease the entire local loop –a requirement that remains in place today.

99. Third, the DC Circuit was also skeptical of the FCC's ruling that virtually every network element, including mass-market switching, had to be offered by incumbents to competitors at regulated rates. The Court instructed the Commission to examine whether the unbundling of switching was required in every geographic market given the investment in switching by many new competitors in many urban markets. When the FCC failed to drop the requirement for unbundling switching services in its revised rules in 2003, the DC Circuit once again instructed the FCC to reconsider in a strongly-worded opinion. (*United States Telecommunications Association v. FCC*, 259 F3d. 572 (2003)) As a result, in 2004 the FCC was forced to eliminate unbundling of mass-market switching and, therefore, the UNE-P. However, competitors could still lease, among other network elements, the copper loop at regulated rates if they wished to offer broadband and any other telecommunications services.

100. Thus, the *Berkman Study* is incorrect when it asserts that:

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[97] *AT&T Corp. v. Iowa Utilities Board*, 525 U.S. 366 (1999). ("The FCC cannot, consistent with the statute, blind itself to the availability of elements outside the incumbent's network. In addition, the FCC's assumption that any increase in cost (or decrease in quality) imposed by denial of a network element renders access to that element 'necessary,' and causes the failure to provide that element to 'impair' the entrant's ability to furnish its desired services, is simply not in accord with the ordinary and fair meaning of those terms.")

> By the fall of 2001 a new FCC had changed course. Between that fall and the spring of 2002, the FCC passed a series of decisions that abandoned the effort to implement open access, and shifted the focus of American policy from the idea of regulated competition within each wire – competition over the copper plant of the telephone company and over the coaxial cable of the cable company – to competition between the owners of the two wires.[98]

To the contrary, neither the FCC nor state regulators in the U.S. "abandoned" local loop unbundling in 2002 or at any time since then. Competitors may still lease the local loop at regulated rates. However, they do not have the right to *share* the loop at even lower regulated rates.

101. It is true, however, that as a result of these changes in U.S. policy, competitors' use of incumbents' unbundled network elements began to decline. A large share of this decline was due to the ending of the UNE-P caused by the FCC's decision to drop network switching from the list of required mandatory unbundled elements. These lines were not being used to offer broadband services, but rather to compete in the provision of traditional telephone services. Many entrants – including Covad, NorthPoint, Rhythms, and Net 2000 – had tried to offer DSL services over unbundled incumbent loops and even *shared* incumbent loops and had failed. Indeed, given the aggressive competition between cable companies and incumbent telephone companies in offering broadband, and despite the best efforts of state and federal regulators to create a favorable regulatory environment and low rates for unbundled elements,

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[98] *Berkman Study* at 82. (*See also* at 77: "Some form of open access regulation has at this point been adopted by every country in the OECD except the United States, Mexico, and the Slovak Republic (which has been in the process of passing unbundling requirements for over two years, but has not yet done so)." The *Berkman Study* then goes on to note that broadband penetration in both Mexico and the Slovak Republic is relatively low, apparently implying that there is a connection between low broadband penetration and the absence of unbundling in these countries. Yet, the *Berkman Study* provides no evidence whatsoever for this conclusion; to the contrary, its regression analysis of the determinants of broadband penetration suggests that penetration in these countries is explained by their relatively low incomes and population densities.)

no U.S. broadband entrant that has relied on unbundled loops has been able to avoid bankruptcy.[99]

102. The *Berkman Study* also presents an incomplete (and, as a result, misleading) discussion of the U.S. decision not to impose open access policies on cable broadband providers, implying that the FCC failed to actively consider imposing such rules on cable operators. As the *Berkman Study* tells it,

> … around 1999-2000, as AT&T purchased major cable systems, a new question emerged – whether cable should be subject to the same kind of open access regulation. In several instances cable franchising authorities tried to do this; but the power to impose open access on cable operators was seen as residing in the FCC, not local authorities.[100]

103. In fact, the FCC actively considered imposing open access as a merger condition at the time of the AT&T-TCI merger. After considering the matter, its Chairman at the time, William Kennard, explained the grounds for his decision to forego such a vast expansion of open access regulation onto the cable pipe. As he said in a September 1999 speech,

> It is easy to say that government should write a regulation, to say that as a broad statement of principle that a cable operator shall not discriminate against unaffiliated Internet service providers on the cable platform. It is quite another thing to write that rule, to make it real and then to enforce it…. So, if we have the hope of facilitating a market-based solution here, we should do it, because the alternative is to go to the telephone world, a world that we are trying to deregulate and just pick up this whole morass of regulation and dump it wholesale on the cable pipe. That is not good for America.[101]

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[99] For a discussion of the causes of the CLECs' demise, *see* Larry F. Darby, Jeffrey A. Eisenach and Joseph S. Kraemer, "The CLEC Experiment: Anatomy of a Meltdown," Progress on Point 9.23, *The Progress & Freedom Foundation* (September 2002); see also Jeffrey A. Eisenach and Janusz R. Mrozek, "Do UNE Rates Reflect Underlying Costs?" CapAnalysis LLC (December 2003).

[100] *Berkman Study* at 82.

[101] William E. Kennard, "Consumer Choice Through Competition: Remarks Before the National Association of Telecommunications Officers and Advisors, 19th Annual Conference," (September 17, 1999) (available at http://www.fcc.gov/Speeches/Kennard/spwek931.html).

C. The *Berkman Study* Mischaracterizes the Policy Consensus Abroad

104. The *Berkman Study* also goes out of its way to characterize foreign views of unbundling, seeking to present a picture of virtual unanimity among foreign regulators about unbundling's virtues, with the U.S. (along with a few others) as "recalcitrant" outliers.

105. While it is undeniably true that unbundling has played a larger role in some other countries than in the U.S., the reason for this result is not – as the *Berkman Study* suggests – because their regulators agree that intramodal competition (supported by unbundling) is generally superior to intermodal competition. To the contrary, there is a wide and deep consensus that conditions among and within countries vary widely, and that intermodal competition is superior *wherever it is possible*.

106. Consider, for example, the June 2009 report of the European Regulators Group (ERG),[102] which is cited numerous times by the *Berkman Study* for the proposition that foreign regulators support the unbundling of broadband networks. In fact, the *ERG Report* emphasizes that economic conditions vary from country to country, that the "economic viability of roll-out strategies is largely influenced by specific local characteristics,"[103] and that "[o]verall, the country cases support the result … that there are significant differences between and within countries, which reflect differences in the economics of NGA networks, resulting from e.g. different densities or loop length but also from the relevance of competitive pressure from cable networks."[104]

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[102] European Regulators Group, *Report on Next Generation Access - Economic Analysis and Regulatory Principles* (June 2009) (hereafter *ERG Report*).

[103] *ERG Report* at 4.

[104] *ERG Report* at 5.

107. For example, the *ERG Report* finds with respect to Korea and Japan that "Due to the high population density and greater proportion of aerial cables… it is relatively cheap to deploy fibre in these countries."[105] The ERG also notes approvingly that "Outside of Europe, AT&T is rolling out a FttCab network and Verizon offers FttH in certain US cities. This is in part because of the poor quality of existing DSL services (due to long copper lines) and because of strong competition from cable operators."[106]

108. While the *ERG Report* acknowledges that various factors affect broadband penetration and investment, and that these factors vary across countries, it is unambiguous in its support for infrastructure competition, finding that "Competition, in particular infrastructure competition, promotes investment as can be seen in countries with high cable penetration. Regulation should set the framework in such a way that it promotes competition."[107] Thus, "Where it is practically and economically feasible to promote infrastructure based competition, this should be the aim of national regulatory authorities (NRAs)."[108]

109. The *Berkman Study* does not stop, however, at mischaracterizing foreign regulators' opinions: It also mischaracterizes their actions. For example, at page 76, the *Berkman Study* lists as a core finding that "Access rules are now being applied to the next generation transition, particularly to fiber," and cites the *ERG Report* in support of this conclusion. In fact, however, the *ERG Report* states plainly that among European countries, "Unbundled fibre access is only available on regulated terms in the Netherlands."[109]

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[105] *ERG Report* at 7.
[106] *ERG Report* at 6.
[107] *ERG Report* at 16.
[108] *ERG Report* at 1.
[109] *ERG Report* at 13.

Moreover, as noted above, the most recent ECTA and IDATE data show that the incumbent in the Netherlands (KPN) currently has no residential fiber loops to unbundle!

VI. CONCLUSIONS

110. In our opinion, for the reasons explained above, the *Berkman Study* does not constitute a sound foundation for decisionmaking by the Commission. The study is neither rigorous nor impartial, but instead presents a highly opinionated, and in many respects demonstrably incorrect, portrayal of the evidence as it relates to the effects of public policy on broadband penetration and other key indicators of performance in the broadband market. In particular, as we have demonstrated above, the weight of the evidence demonstrates that mandatory unbundling has reduced broadband penetration and deterred investment in broadband telecommunications infrastructure, and that mandatory unbundling of Next Generation infrastructures would have similar effects.

Robert W. Crandall
Everett M. Ehrlich
Jeffrey A. Eisenach

November 16, 2009

Exhibit A: Crandall Qualifications

R O B E R T W. C R A N D A L L

CURRENT POSITION:

Non-Resident Senior Fellow, The Brookings Institution

ADDRESS:

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Washington, DC 20036

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FIELDS OF SPECIALIZATION:

Industrial Organization, Antitrust Policy, Regulation

PREVIOUS POSITIONS:

Adjunct Professor, School of Public Affairs, University of Maryland, 1987 - 1993
Deputy Director, Council on Wage and Price Stability, 1977 - 1978
Acting Director, Council on Wage and Price Stability, 1977
Adjunct Associate Professor of Economics, George Washington University, 1975 - 1977
Assistant Director, Council on Wage and Price Stability, 1975 - 1977
Associate Professor of Economics, M.I.T., 1972 - 1974
Assistant Professor of Economics, M.I.T., 1966 - 1972
Johnson Research Fellow, The Brookings Institution, 1965 - 1966
Instructor, Northwestern University, 1964 - 1965
Consultant to Environmental Protection Agency, Antitrust Division Federal Trade Commission, Treasury Department, various years

EDUCATION:

Ph.D., Economics, Northwestern University, 1968
M.A., Economics, Northwestern University, 1965
A.B., Economics, University of Cincinnati, 1962

MEMBERSHIPS:

American Economic Association
Board of Directors, Baltimore Life Insurance Company

PUBLICATIONS:

Books:

Competition and Chaos: U.S. Telecommunications since the 1996 Act. Washington, DC: The Brookings Institution, 2005.

Broadband: Should We Regulate High-Speed Internet Access? (edited with James Alleman), AEI Brookings Joint Center for Regulatory Studies, 2002.

Telecommunications Liberalization on Two Sides of the Atlantic. (with Martin Cave) AEI Brookings Joint Center for Regulatory Studies, 2001.

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Articles, Reports, and Contributions to Edited Volumes:

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Exhibit B: Ehrlich Qualifications

Everett M. Ehrlich

Dr. Everett M. Ehrlich is one of the nation's leading business economists. His firm, ESC Company, combines economic analysis, business development, and communications skills to solve a wide range of business problems. ESC's diverse clientele have included leading firms in the financial, accounting, pharmaceutical, automotive, and other industries, and such diverse organizations as the Pew Center for Global Climate Change and the Major League Baseball Players Association. He also recently served as Executive Director of the CSIS Commission on Public Infrastructure under co-chairmen Felix Rohatyn and Warren Rudman; a bipartisan bill to enact their recommendations was introduced in the 110th Congress.

Dr. Ehrlich served in the Clinton Administration as Under Secretary of Commerce for Economic Affairs, the principal economic policy official for Commerce Secretaries Brown and Kantor and chief executive of the nation's statistical system. As such, he led the first comprehensive strategic review of the nation's economic statistics in four decades, leading to a major modernization of featured measures of the economy. He supervised the redesign of the 2000 decennial census. He co-chaired the White House working group on the restructuring of the U.S. economy in the face of information technology, was a leader in the U.S.' planning effort of the two G-7 "Jobs Summits," and oversaw the Administration's economic analysis of global climate change.

Prior to his service as Under Secretary, Dr. Ehrlich was Vice-President for Economic and Financial Planning, and for Strategic Planning, of Unisys Corporation, from 1988 to 1993. As such, he had responsibilities concerning corporate development and finance, formulating business strategy, and economic forecasting. He reported directly to two chairmen of the company. He has also been the Senior Vice-President and research director of the business-based think tank, the Committee for Economic Development.

Dr. Ehrlich earlier served as Assistant Director of the Congressional Budget Office, where he directed the CBO program in trade and technology, infrastructure and space transportation, energy and the environment, and agriculture. He joined CBO in 1977, after having served as a Legislative Aide to Congressman John Conyers, Jr., and having briefly taught economics at the university level.

Dr. Ehrlich is the author of two critically-acclaimed novels: *Big Government* (1998), and *Grant Speaks* (2000), both by Warner Books. He was, for eight years, a regular economics commentator on National Public Radio's *Morning Edition*, and his writings have appeared in *The Financial Times*, *Investors Business Daily*, *The Christian Science Monitor*, *The Washington Post*, *The International Economy*, *The New York Review of Books*¸ and other publications.

Dr. Ehrlich was born in New York City in 1950 and is a product of its public schools. He received a B.A. in 1971 from S.U.N.Y. Stony Brook and a Ph.D. in economics in 1975 from the University of Michigan. He lives with his family in Bethesda, Maryland, where he has coached little league, acted in children's theater, been wardrobe master for the high school chorus, and waits for the Washington Nationals to win the World Series.

Exhibit C: Eisenach Qualifications

JEFFREY AUGUST EISENACH

Education

Ph.D. in Economics, University of Virginia, 1985
B.A. in Economics, Claremont McKenna College, 1979

Professional Experience

Chairman and Managing Partner, Empiris LLC, September 2008-present
Chairman, Criterion Economics, LLC, June 2006-September 2008
Chairman, The CapAnalysis Group, LLC, July 2005-May 2006
Executive Vice Chairman, The CapAnalysis Group, LLC, February 2003-July 2005
President, The Progress & Freedom Foundation, June 1993-January 2003
Executive Director, GOPAC, July 1991-May 1993
President, Washington Policy Group, Inc., March 1988-June 1991
Director of Research, Pete du Pont for President, Inc., September 1986-February 1988
Executive Assistant to the Director, Office of Management and Budget, 1985-1986
Special Advisor for Economic Policy and Operations, Office of the Chairman, Federal Trade Commission, 1984-1985
Economist, Bureau of Economics, Federal Trade Commission, 1983-1984
Special Assistant to James C. Miller III, Office of Management and Budget/Presidential Task Force on Regulatory Relief, 1981
Research Associate, American Enterprise Institute, 1979-1981
Consultant, Economic Impact Analysts, Inc., 1980
Research Assistant, Potomac International Corporation, 1978

Teaching Experience

Adjunct Professor, George Mason University School of Law, 2000-present (Courses Taught: Regulated Industries; Perspectives on Government Regulation; The Law and Economics of the Digital Revolution)
Adjunct Lecturer, Harvard University, John F. Kennedy School of Government, 1995-1999 (Course Taught: The Role of Government in the 21st Century)
Adjunct Professor, George Mason University, 1989 (Course Taught: Principles of Economics)
Adjunct Professor, Virginia Polytechnic Institute and State University, 1985, 1988 (Courses Taught: Graduate Industrial Organization, Principles of Economics)
Instructor, University of Virginia, 1983-1984 (Courses Taught: Value Theory, Antitrust Policy)
Teaching Assistant, University of Virginia, 1982-1983 (Courses Taught: Graduate Microeconomics, Undergraduate Macroeconomics)

Awards, Activities and Concurrent Positions

Member, Economic Club of Washington, 2009-
Member, Board of Directors, Intelligent Grid Solutions, 2009-
Member, Board of Directors, PowerGrid Communications, 2008-2009
Member, Board of Advisors, Washington Mutual Investors Fund, 2008-present
Member, Board of Advisors, Pew Project on the Internet and American Life, 2002-present
Member, Board of Directors, The Progress & Freedom Foundation, 1993-2009
Member, Attorney General's Identity Theft Task Force, Virginia, 2002

Member of the Board of Directors, Privacilla.com, 2002-2003
Member, Executive Board of Advisors, George Mason University Tech Center, 2001-2004
Contributing Editor, *American Spectator*, 2001-2002
Member, Bush-Cheney Transition Advisory Committee on the FCC, 2001
Member, Governor's Task Force on E-Communities, State of Virginia, 2000-2001
Member, 2000-2001 Networked Economy Summit Advisory Committee, 1999-2001
Member, Board of Directors, Internet Education Foundation, 1998-2003
Member, Internet Caucus Advisory Committee, 1998-2003
Member, American Assembly Leadership Advisory Committee, 1996 -2002
Member, Commission on America's National Interests, 1995-2000
Adjunct Scholar, Hudson Institute, 1988-1991
Visiting Fellow, Heritage Foundation, 1988-1991
President's Fellowship, University of Virginia, 1981-1984
Earhart Foundation Fellowship, University of Virginia, 1981-1983
Member, Reagan-Bush Transition Team on the Federal Trade Commission, 1981
Henry Salvatori Award, Claremont Men's College, 1979
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Exhibit D: Regression Data and Variable Description

Table D.1:
Variables, Descriptions, and Sources

Variable Name	Description	Sources
Country	Country Name	NA
Year	Year	NA
qtot	Broadband penetration per 100 persons	OECD
yeardsl	Year in which DSL was first offered	Incumbent carrier filings and news reports
monthdsl	Month in which DSL was first offered	Incumbent carrier filings and news reports
dslyears	Years since DSL was first offered	Constructed using *yeardsl* and *monthdsl*
gdp	GDP per capita in US PPP	OECD
pops_mils	Population in millions	OECD
pop_density	Population density	OECD
State owned	Whether the network is state owned	Company and country sources
guyrs	Formulation of GUyrs used in Boyle-Howell-Zhang and in many *Berkman Study* regressions. This formulation does *not* contain negative values in years before unbundling.	*de Ridder Report*, based on 2005 OECD data, Table 2.10.
guyrs_new	GUyrs values used by *Berkman Study*	Berkman Study, Table 4.8

Source Notes: OECD Broadband data for the most recent report (2008) is available at: http://www.oecd.org/document/54/0,3343,en_2649_34225_38690102_1_1_1_1,00.html. Historic reports, which were used to compile data before 2004, are available at: http://www.oecd.org/statisticsdata/0,3381,en_2649_34225_1_119656_1_1_1,00.html.

Table D.2:
Panel Data for 2001 to 2006 OECD Regression Analysis

country	year	qtot	yeardsl	monthdsl	dslyears	gdp	pops_mils	pop_density	state owned	guyrs	guyrs new
Australia	2001	0.846805	2000	8	1.333333	26275.82	19.443	2.526978	0	2	0
Australia	2002	1.84032	2000	8	2.333333	27514	19.672	2.556656	0	3	0
Australia	2003	3.493849	2000	8	3.333333	28621.81	19.907	2.586855	0	4	0
Australia	2004	7.663334	2000	8	4.333333	30118.42	20.143	2.615363	0	5	0
Australia	2005	13.60262	2000	8	5.333333	31425.49	20.395	2.646213	0	6	1
Australia	2006	18.332	2000	8	6.333333	32937.8	20.65	2.687997	0	7	2
Austria	2001	3.599902	1999	11	2.083333	29812.48	8.043	97.55022	0	3	1
Austria	2002	5.606536	1999	11	3.083333	30438.6	8.084	98.04292	0	4	2
Austria	2003	7.637688	1999	11	4.083333	31293.29	8.118	98.45724	0	5	3
Austria	2004	10.60978	1999	11	5.083333	32738.36	8.175	99.14736	0	6	4
Austria	2005	14.35259	1999	11	6.083333	34173.05	8.233	99.85809	0	7	5
Austria	2006	16.7087	1999	11	7.083333	36031.43	8.263	100.2183	0	8	6
Belgium	2001	4.368596	1999	10	2.166667	28533.88	10.31	340.2911	0	1	0
Belgium	2002	8.670581	1999	10	3.166667	29332.55	10.355	341.8128	0	2	0
Belgium	2003	11.69562	1999	10	4.166667	30112.68	10.396	343.2379	0	3	0
Belgium	2004	15.54136	1999	10	5.166667	31557.1	10.446	344.7278	0	4	0
Belgium	2005	18.16631	1999	10	6.166667	32807.75	10.511	346.6308	0	5	0
Belgium	2006	22.34282	1999	10	7.166667	34477.59	10.576	349.8511	0	6	1
Canada	2001	8.848163	2000	4	1.666667	28668.61	30.974	3.418031	0	5	1
Canada	2002	12.11409	2000	4	2.666667	29693.77	31.323	3.448833	0	6	2
Canada	2003	15.06256	2000	4	3.666667	30575.72	31.633	3.478305	0	7	3
Canada	2004	17.6077	2000	4	4.666667	32162.23	31.947	3.517784	0	8	4
Canada	2005	20.72957	2000	4	5.666667	33778.91	32.258	3.551874	0	9	5
Canada	2006	24.28547	2000	4	6.666667	35494.47	32.581	3.582885	0	10	6
Czech Republic	2001	0.117624	2000	3	1.75	16500.77	10.224	132.3153	0	0	0
Czech Republic	2002	0.165605	2000	3	2.75	17146.6	10.201	132.0176	0	0	0
Czech Republic	2003	0.475377	2000	3	3.75	18140.65	10.202	132.0305	0	0	0
Czech Republic	2004	2.500245	2000	3	4.75	19432.28	10.207	132.1114	0	1	1
Czech Republic	2005	6.458863	2000	3	5.75	21191.92	10.229	132.463	0	2	2
Czech Republic	2006	11.07196	2000	3	6.75	23100.24	10.24	132.5395	0	3	3
Denmark	2001	4.435025	1999	6	2.5	29944.8	5.349	126.2098	0	4	4
Denmark	2002	8.245852	1999	6	3.5	30501.2	5.368	126.6617	0	5	5
Denmark	2003	13.10354	1999	6	4.5	31272.91	5.384	126.9662	0	6	6
Denmark	2004	18.95539	1999	6	5.5	32677.87	5.398	127.2962	0	7	7
Denmark	2005	24.92	1999	6	6.5	34669.02	5.411	127.645	0	8	8
Denmark	2006	31.78843	1999	6	7.5	36548.6	5.427	127.9048	0	9	9
Finland	2001	1.308951	1999	9	2.25	27233.92	5.188	17.03272	0	6	6
Finland	2002	5.450875	1999	9	3.25	28096.38	5.201	17.07409	0	7	7
Finland	2003	9.482064	1999	9	4.25	29133.64	5.213	17.1148	0	8	8
Finland	2004	14.9203	1999	9	5.25	30922.34	5.228	17.16453	0	9	9
Finland	2005	22.38661	1999	9	6.25	32677.7	5.246	17.22348	0	10	10
Finland	2006	27.1386	1999	9	7.25	34818.84	5.245	17.21987	0	11	11
France	2001	1.048001	2000	6	1.5	25839.19	61.12	107.603	0	1	0
France	2002	2.763338	2000	6	2.5	26405.85	61.53	108.3414	0	2	1
France	2003	5.941943	2000	6	3.5	27094.95	61.932	109.3526	0	3	2
France	2004	10.45725	2000	6	4.5	28252.42	62.324	110.0184	0	4	3
France	2005	15.06824	2000	6	5.5	29276.33	62.702	110.6581	0	5	4
France	2006	20.12535	2000	6	6.5	30693.1	63.033	114.5846	0	6	5
Germany	2001	2.349625	1999	4	2.666667	26341.74	82.44	236.0667	0	4	0
Germany	2002	3.945103	1999	4	3.666667	26773.31	82.537	236.5685	0	5	0
Germany	2003	5.588083	1999	4	4.666667	27278.27	82.532	236.6631	0	6	0
Germany	2004	8.369575	1999	4	5.666667	28413.43	82.501	236.5922	0	7	0
Germany	2005	12.98336	1999	4	6.666667	29549.53	82.438	236.4578	0	8	0
Germany	2006	18.19027	1999	4	7.666667	31095.08	82.293	235.9521	0	9	1

country	year	qtot	yeardsl	monthdsl	dslyears	gdp	pops_mils	pop_density	state owned	guyrs	guyrs new
Hungary	2001	0.256053	2000	9	1.25	13876.44	10.2	113.6753	0	0	0
Hungary	2002	0.646782	2000	9	2.25	14694.23	10.175	113.352	0	1	1
Hungary	2003	1.994175	2000	9	3.25	15564.17	10.142	113.0278	0	2	2
Hungary	2004	3.569184	2000	9	4.25	16884.15	10.117	112.7772	0	3	3
Hungary	2005	6.33983	2000	9	5.25	18145.25	10.098	112.5661	0	4	4
Hungary	2006	9.585401	2000	9	6.25	19559.43	10.077	112.454	0	5	5
Iceland	2001	3.675202	2000	4	1.666667	30940.94	0.287	2.842893	0	2	2
Iceland	2002	8.445224	2000	4	2.666667	30947.75	0.288	2.872818	0	3	3
Iceland	2003	14.25826	2000	4	3.666667	32325.66	0.291	2.88826	0	4	4
Iceland	2004	18.2045	2000	4	4.666667	35606.13	0.294	2.913677	0	5	5
Iceland	2005	26.36921	2000	4	5.666667	37898.05	0.3	2.9601	0	6	6
Iceland	2006	28.82951	2000	4	6.666667	40276.91	0.302	3.012469	0	7	7
Ireland	2001	0.010419	2001	10	0.166667	33243.23	3.847	56.12498	0	1	0
Ireland	2002	0.271197	2001	10	1.166667	35223.3	3.917	57.0736	0	2	0
Ireland	2003	0.828113	2001	10	2.166667	36935.13	3.979	58.00116	0	3	0
Ireland	2004	3.322034	2001	10	3.166667	38985.56	4.044	59.05719	0	4	0
Ireland	2005	6.606889	2001	10	4.166667	41492.43	4.131	60.37306	0	5	0
Ireland	2006	12.20297	2001	10	5.166667	44087.2	4.235	61.47482	0	6	1
Italy	2001	0.723652	2000	2	1.833333	26815.34	57.229	193.7394	0	1	1
Italy	2002	1.681979	2000	2	2.833333	27303.95	57.382	194.3402	0	2	2
Italy	2003	4.134502	2000	2	3.833333	27887.09	57.399	195.8609	0	3	3
Italy	2004	8.081182	2000	2	4.833333	28962.89	57.442	197.8012	0	4	4
Italy	2005	11.7677	2000	2	5.833333	29502.32	58.077	199.2691	0	5	5
Italy	2006	14.23954	2000	2	6.833333	30732.01	58.275	198.1402	0	6	6
Japan	2001	2.232016	1999	11	2.083333	26349.92	127.132	348.8313	0	5	2
Japan	2002	6.21777	1999	11	3.083333	26789.35	127.4	349.6461	0	6	3
Japan	2003	10.89797	1999	11	4.083333	27794.27	127.634	350.3978	0	7	4
Japan	2004	14.96068	1999	11	5.083333	29219.38	127.734	350.5158	0	8	5
Japan	2005	18.1459	1999	11	6.083333	30888.67	127.752	350.546	0	9	6
Japan	2006	20.69457	1999	11	7.083333	32646.52	127.748	350.4746	0	10	7
Korea	2001	18.4699	1999	6	2.5	17075.98	47.357	479.6264	0	0	5
Korea	2002	24.31035	1999	6	3.5	18481.8	47.622	482.2762	0	1	6
Korea	2003	26.1209	1999	6	4.5	19363.19	47.859	484.6473	0	2	7
Korea	2004	27.02542	1999	6	5.5	20776.92	48.039	487.0066	0	3	8
Korea	2005	28.68983	1999	6	6.5	22207.8	48.138	489.1537	0	4	9
Korea	2006	29.01406	1999	6	7.5	23926.12	48.297	489.1826	0	5	10
Luxembourg	2001	0.278912	2000	10	1.166667	62231.18	0.442	169.6911	1	1	0
Luxembourg	2002	1.537651	2000	10	2.166667	64929.06	0.446	171.2355	1	2	0
Luxembourg	2003	3.460222	2000	10	3.166667	67090.58	0.45	173.7247	1	3	0
Luxembourg	2004	9.636542	2000	10	4.166667	71404.84	0.453	175.0181	1	4	0
Luxembourg	2005	14.47915	2000	10	5.166667	75863.29	0.457	176.3358	1	5	0
Luxembourg	2006	21.00719	2000	10	6.166667	80471.4	0.461	177.9923	1	6	1
Mexico	2001	0.112069	2000	7	1.416667	9120.312	99.377	51.86494	0	0	0
Mexico	2002	0.245927	2000	7	2.416667	9217.483	100.819	52.39318	0	0	0
Mexico	2003	0.417083	2000	7	3.416667	9411.607	102.291	52.9268	0	0	0
Mexico	2004	1.008547	2000	7	4.416667	10107.27	102.05	53.46586	0	0	0
Mexico	2005	2.216157	2000	7	5.416667	10615.09	103.089	54.01041	0	0	0
Mexico	2006	2.843348	2000	7	6.416667	11249.43	104.139	54.56046	0	1	1
Netherlands	2001	3.829361	1999	11	2.083333	29491.04	16.046	473.6154	0	5	1
Netherlands	2002	7.036165	1999	11	3.083333	29837.79	16.149	476.6497	0	6	2
Netherlands	2003	11.79241	1999	11	4.083333	30430.9	16.225	478.904	0	7	3
Netherlands	2004	18.95774	1999	11	5.083333	31806.7	16.276	480.5706	0	8	4
Netherlands	2005	25.21648	1999	11	6.083333	33198.45	16.311	481.6957	0	9	5
Netherlands	2006	30.99566	1999	11	7.083333	35077.75	16.345	482.438	0	10	6
New Zealand	2001	0.729325	2000	8	1.333333	20502.72	3.886	14.48002	0	0	0
New Zealand	2002	1.612214	2000	8	2.333333	21548.83	3.942	14.69868	0	0	0
New Zealand	2003	2.569094	2000	8	3.333333	22375.55	4.01	14.96026	0	0	0
New Zealand	2004	4.719227	2000	8	4.333333	23700.51	4.062	15.15504	0	0	0

country	year	qtot	yeardsl	monthdsl	dslyears	gdp	pops_mils	pop_density	state owned	guyrs	guyrs new
New Zealand	2005	9.119727	2000	8	5.333333	24738.02	4.101	15.29497	0	0	0
New Zealand	2006	11.83165	2000	8	6.333333	25531.12	4.145	15.467	0	1	1
Norway	2001	1.865131	2000	11	1.083333	35764.22	4.519	14.83173	0	1	1
Norway	2002	4.197929	2000	11	2.083333	36596.63	4.543	14.91389	0	2	2
Norway	2003	8.176582	2000	11	3.083333	37581.63	4.569	15.0023	0	3	3
Norway	2004	15.20094	2000	11	4.083333	39596.89	4.597	15.0912	0	4	4
Norway	2005	22.622	2000	11	5.083333	41629.93	4.606	15.19423	0	5	5
Norway	2006	26.83757	2000	11	6.083333	43574.41	4.637	15.23925	0	6	6
Poland	2001	0.056148	2000	11	1.083333	10865.47	38.248	125.681	0	0	0
Poland	2002	0.298312	2000	11	2.083333	11217.42	38.23	124.8229	0	0	0
Poland	2003	0.778167	2000	11	3.083333	11902.17	38.205	124.7257	0	1	0
Poland	2004	2.143989	2000	11	4.083333	12893.44	38.182	125.4755	0	2	0
Poland	2005	2.412809	2000	11	5.083333	13741.09	38.165	124.5893	0	3	0
Poland	2006	7.177496	2000	11	6.083333	14880.17	38.097	124.3659	0	4	1
Portugal	2001	0.987139	2000	11	1.083333	20045.26	10.294	112.4911	0	1	0
Portugal	2002	2.511885	2000	11	2.083333	20409.01	10.366	113.315	0	2	0
Portugal	2003	4.806992	2000	11	3.083333	20453.07	10.445	114.1098	0	3	1
Portugal	2004	7.890145	2000	11	4.083333	21154.37	10.509	114.7756	0	4	2
Portugal	2005	11.04745	2000	11	5.083333	21790.82	10.549	115.2945	0	5	3
Portugal	2006	13.45131	2000	11	6.083333	22677.46	10.56	115.4098	0	6	4
Spain	2001	1.177872	1999	9	2.25	22560.49	40.734	81.68268	0	1	0
Spain	2002	2.984174	1999	9	3.25	23271.92	41.255	82.78685	0	2	1
Spain	2003	5.408101	1999	9	4.25	24043.69	42.003	84.14194	0	3	2
Spain	2004	8.061571	1999	9	5.25	25081.66	42.69	85.52184	0	4	3
Spain	2005	11.50802	1999	9	6.25	26295.6	43.389	86.93714	0	5	4
Spain	2006	15.11046	1999	9	7.25	27522.08	44.144	88.43126	0	6	5
Sweden	2001	5.20571	1999	6	2.5	26968.32	8.905	21.67524	0	1	1
Sweden	2002	8.161345	1999	6	3.5	27896.05	8.934	21.74835	0	2	2
Sweden	2003	11.1508	1999	6	4.5	28865.17	8.967	21.82633	0	3	3
Sweden	2004	14.87547	1999	6	5.5	30673.57	9.003	21.91405	0	4	4
Sweden	2005	20.77519	1999	6	6.5	32324.69	9.039	21.99215	0	5	5
Sweden	2006	26.40678	1999	6	7.5	34409.27	9.091	22.15534	0	6	6
Switzerland	2001	1.959452	2000	10	1.166667	31090.39	7.211	180.7452	0	0	0
Switzerland	2002	5.644284	2000	10	2.166667	31645.49	7.23	182.1174	0	0	0
Switzerland	2003	10.55475	2000	10	3.166667	32157.79	7.247	183.4739	0	0	0
Switzerland	2004	17.66716	2000	10	4.166667	33686.99	7.261	184.7395	0	0	0
Switzerland	2005	23.84787	2000	10	5.166667	35289.9	7.274	185.9275	0	0	0
Switzerland	2006	27.67285	2000	10	6.166667	37368.87	7.287	182.175	0	1	1
Turkey	2001	0.015617	1999	6	2.5	6178.182	67.294	89.04149	1	0	0
Turkey	2002	0.036648	1999	6	3.5	6676.541	68.39	90.46685	1	0	0
Turkey	2003	0.276441	1999	6	4.5	7100.696	69.478	91.87791	1	0	0
Turkey	2004	0.705473	1999	6	5.5	7833.587	70.551	92.44669	1	0	0
Turkey	2005	2.123083	1999	6	6.5	8538.674	71.605	93.6359	1	0	0
Turkey	2006	3.800922	1999	6	7.5	9106.932	72.564	94.28426	1	1	1
United Kingdom	2001	0.583217	2000	8	1.333333	27970.06	59.113	245.94	0	1	0
United Kingdom	2002	2.316143	2000	8	2.333333	28942.1	59.322	245.12	0	2	0
United Kingdom	2003	5.390989	2000	8	3.333333	30227.52	59.554	246.2774	0	3	0
United Kingdom	2004	10.35532	2000	8	4.333333	31950.35	59.834	247.3207	0	4	0
United Kingdom	2005	16.31788	2000	8	5.333333	33313.59	60.218	248.9418	0	5	0
United Kingdom	2006	21.44873	2000	8	6.333333	35051.39	60.533	250.2087	0	6	1
United States	2001	4.343838	1999	9	2.25	34845.74	285.365	31.14173	0	6	0
United States	2002	6.711872	1999	9	3.25	35650.68	288.331	31.47474	0	7	0

country	year	qtot	yeardsl	monthdsl	dslyears	gdp	pops_mils	pop_density	state owned	guyrs	guyrs new
United States	2003	9.571342	1999	9	4.25	36956.1	291.194	31.74116	0	8	0
United States	2004	12.75508	1999	9	5.25	39116.76	293.978	32.05173	0	9	0
United States	2005	16.32303	1999	9	6.25	41196.51	296.852	32.35243	0	10	0
United States	2006	20.26841	1999	9	7.25	43444.15	299.715	32.71312	0	11	1

Exhibit E: Summary of Studies of the Effect of Unbundling on Investment

	Data and period	Main results regarding regulation and investment	Methodology
Hausman (1998)	US 1988–1994	The adoption of TELRIC approach of mandatory unbundling is a failure because it neglects the important role of sunk and irreversible investments in telecommunications. The regulated price for telecommunications services from new investment is set too low and both ILECs and CLECs are discouraged from new investments in infrastructure.	Numeric examples
Hausman (1998)	US	The failure to recognize the sunk cost character of network investment leads LLU to have negative economic incentives for innovation and for new investment. The FCC's short sight on static cost efficiency questions resulted in its failure to account for the large gains in dynamic economic efficiency arising from new investment.	Numeric examples
Christodoulou and Vlahous (2001)	UK	A "mix" of infrastructure and service competition with a gradually increasing price of the UNE from an initially low level to forward-looking costs, like the one promoted in the Netherlands, stimulates investment by both incumbents and entrants. Therefore the introduction of sunset clauses to encourage the firm's investment incentive is supported.	Industry simulation
Chang, Koski and Majumdar (2003)	European countries 1997	The adoption of either the fully distributed costs method or LRIC method for determining access charges has not induced investment in networks, while an ex post direct cost method does increase investments to above-average levels.	Uni-variate statistical tests
Crandall and Singer (2003)	US RBOCs 1996–2002	For every line lost to a CLEC via UNE-P at the regulated rates of that time, the RBOC loses roughly $18.50 in revenue, $15.50 in earnings, and $10 in operating cash flows each month, which in turn reduces the ILEC's capital spending.	A four-step procedure
Ingraham and Sidak (2003)	US ILECs 1996–2002	It is affirmed that mandatory unbundling would increase the volatility of the ILECs' stock returns during times of recession and therefore increase their equity costs, which confirms the statement in Jorde, Sidak, and Teece (2000) that mandatory unbundling at TELRIC prices has decreased the ILEC's incentives to invest in their own networks by raising their capital cost.	Least-squares regressions under different time periods
Phoenix Center Policy Bulletin No.5 (2003)	US BOCs 2000–2002	A positive relationship between UNE-P competition and BOC average net investment is claimed, that each UNE-P access line increases BOC net investment by $759 per year, or about 6.4% per year in aggregate.	Three different versions of a first-difference equation
Phoenix Center Policy Bulletin No.6 (2003)	US BOCs 2000–2002	All the results from different specifications are statistically significantly consistent with the conclusion that UNE-P competition increases Bell Company's investment in local telecommunications plant.	Twenty different model specifications, mostly using weighted least squares
Crandall, Ingraham and Singer (2004)	US state-level 2000–2003	Mandatory unbundling decreases facilities-based competition in the short term. Facilities-based lines growth relative to UNE growth is faster in states where the cost of UNEs is higher relative to the cost of facilities based investment. Hence the data do not support the notion that low UNE rates stimulate future facilities-based investment.	Generalized least-squares technique
Ford and Spiwak (2004)	US 2002 and 2003	Unbundled loop prices based on TELRIC are associated with increased availability of broadband services and increased availability of competitive broadband services; hence wholesale network access requirements like unbundling do not dampen broadband availability or investment incentives.	Minimum logit χ^2 method
Hausman and Sidak (2005)	US, UK, New Zealand, Canada, and Germany 1993–2003	Evidence in all the countries shows a decrease in ILECs' capital expenditure, and the stepping-stone hypothesis fails to be substantiated in that CLECs are not encouraged to invest in facilities-based investment.	Case study by country with descriptive statistics
Hazlett and Bazelon (2005)	US state-level 1999–2004	Increases in UNE-P lines are connected with decreases in CLEC-owned lines after some lagged time. There is no statistical support for the proposition that regulated unbundling leads to facilities-based competition and the stepping-stone theory is rejected.	A multivariate regression model estimated using an autoregression correction
Zarakas et al. (2005)	US ILECs and CATV providers 2000–2003	Mandatory sharing of incumbent facilities blunts incentives to make durable investments and diminishes aggregate investment in local exchange infrastructure in general, and broadband facilities in particular.	A dynamic oligopoly model with parameters calibration and investment simulation
Wallsten (2006)	30 OECD countries 1999–2003	It appears that very extensive unbundling mandates and some types of price regulation can reduce broadband investment incentives, though regulations ensuring easier interconnection with the incumbent can increase investment.	Ordinary least-squares regressions controlling for country and year dummies
Willig (2006)	US ILECs	It refutes the Investment Deterrence Hypothesis and supports the Competitive Stimulus Hypothesis, indicating that the relationship	A reduced-form regression and a structural-form regression

	Data and period	Main results regarding regulation and investment	Methodology
		between TELRIC-based UNE pricing and ILEC investment is negative.	
Fevrier and Sraer (2007)	French 2005	Regulation that provides proper incentives to the entrants to build their own network accelerates the increase of Internet consumption and thus sustains broadband adoption.	A structural approach with two-stage estimation
Höffler (2007)	16 European countries 200–2004	Infrastructure competition between DSL and cable TV had a significant and positive impact on the broadband penetration. However, considering the cable investment, infrastructure competition has probably not been welfare enhancing.	Ordinary least-squares regressions with time dummies and IV estimations
Pindyck (2007)	US	It confirms Hausman's statement that network-sharing rules under the 1996 Act ignore the importance of the irreversibility of capital investment, thus playing a disincentive role in investment.	Numeric examples
Waverman and Dagupta (2006)	12 European countries 2002–2006	Reducing LLU prices by 10% causes the subscriber share of alternative infrastructure to fall by 18%, and ultimately results in lower investment in alternative access platforms. Thus access regulation has a substantial cost in that it negatively affects investment in alternative and new access infrastructures.	A system of estimation equations and simulation
Bouckaert, Van Dijk and Verboven (2008)	20 OECD countries 2003–2008	Inter-platform competition has been a main driver of broadband penetration. Intra-platform competition, on the contrary, is found to have an insignificant, or even negative impact on broadband penetration. Extensive unbundling mandates on the incumbent's DSL network may have adverse investment incentives. Therefore, promoting inter-platform competition is the key policy driver to spur broadband investment.	Random effects estimation
Friederiszick, Grajek and Roller (2008)	25 European countries 1997–2006	Unbundling discourages infrastructure investment by entrants in fixed-line telecommunications. The effect simulated is a lost investment of 25.1% of the entrants' infrastructure stock immediately and 111.5% in 5 years. Significant change in incumbents' investment in fixed-line services is not found.	A partial adjustment regression with structurally modeled dynamics of investment process
Jung et al. (2008)	US ILECs and CLECs 1997–2002	Investments by ILECs are positively related to the market share of CLECs and negatively to the absolute number of CLECs. However, after checking for dynamic persistence, the competitive effect turns out to have a weak effect on ILECs' investment.	Two models: fixed effects estimation and dynamic GMM estimation
Wallsten and Hausladen (2009)	27 European countries 2002–2007	A significant negative correlation between the number of unbundled (DSL) connections per capita and the number of fiber connections is found and the negative impact of unbundling policies on new infrastructure investments is confirmed.	Separate regressions for incumbents and entrants controlling for country- and time-fixed effects

Source: *Cambini and Jiang (2009)*